

ANNUAL REVIEW OF PSYCHOLOGY

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VOLUME 9

1958

ANNUAL REVIEWS, INC.
PALO ALTO, CALIFORNIA, U.S.A.

5150.08
A 615
V. 9
1958
ANNUAL REVIEWS, INC.
PALO ALTO, CALIFORNIA, U.S.A.

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Library of Congress Catalog Card Number: 50-13143

FOREIGN AGENCY

Maruzen Company, Limited
6, Tori-Nichome Nihonbashi
Tokyo

PRINTED AND BOUND IN THE UNITED STATES OF AMERICA BY
GEORGE BANTA COMPANY, INC.

PREFACE

In the Preface of the immediately preceding volume it was stated that the functions the *Annual Review of Psychology* is attempting to perform were to be reconsidered during 1956-1957. It can now be said that some progress was indeed made along this line and next May the Editorial Committee expects to meet for a double-length session to continue deliberation. The Committee conducted an informal poll of a small sample of its readers during the spring of 1957 and appeared to find that its public, so far as interests are concerned, is a rather heterogeneous one. At one extreme are the specialists who read but few chapters and to whom the bibliographies are of prime importance. The members of this group generally desire careful assessment of the articles, although for many the completeness of coverage is more important. At the other extreme are the readers who skim all or at least a majority of the chapters in their endeavor to keep abreast of the advances in the discipline. Bibliographies for this group of readers are of far lesser importance.

With the readers' wants so diverse it was to have been expected that the majority of the ideas advanced by the Committee for possible improvement of future volumes have both supporters and opponents, often in almost equal numbers. However, the readers appear to be in substantial agreement with the notion that the *Annual Review* might well broaden the interpretation of the "annual" segment of its title. Instead of regarding its task exclusively as that of assessing the researches which have appeared in a given year and of keeping to an unchanging set of chapter areas, it might fractionate at least a portion of the topics and arrange to have the subareas reviewed at intervals longer than a year. Each volume would still be an annual but only in the sense that a yearly survey of psychological science would be attempted.

Accepting this shift in plan, the Editorial Committee is making its first fairly drastic changes in 1960's Volume 11, the volume the committee planned during its September, 1957, meetings. Volume 10 will keep, in the main, to the *Annual Review's* traditional pattern. The current volume, 9, omits treatment of somesthesis and individual differences under these captions and introduces chapters on engineering psychology and perception. Through the aid of a contract with the National Science Foundation the Annual Reviews, Inc., has been able to facilitate the efforts of several of its *Review* committees in securing the services of Americans or Western Europeans with the requisite knowledge and language facility to write chapters on the contemporary state of Russian science. The *Annual Review of Psychology* has taken advantage of this opportunity and is happy to be able to present to its readers Professor Mintz's chapter on "Recent Developments in Psychology in the U.S.S.R." in the present volume.

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From time to time readers have complained about the "unpsychological" look of the *Annual Review's* bibliographies and subject index. Happily, one of the three items of this complaint has now been substantially met—that of including article as well as book titles in the bibliographies. All but two of this year's chapters carry article titles and from now on all will do so. The attempt has been made this year to compile a subject index more like that of the *Psychological Abstracts*. While this has not proved easy to do, the policy will be continued. But so far it has not been possible to abbreviate and otherwise arrange the bibliography items as do the other psychological journals. It is the feeling of the Board of Directors of the Annual Reviews, Inc., that editorial operations are simplified by standardizing abbreviations and style throughout the nine *Reviews*.

On January 1, 1957, Robert L. Thorndike's term on the Editorial Committee was completed and he was replaced by Lloyd G. Humphreys. Mrs. Adele Fumino served throughout the year as Editorial Assistant and Miss Robbie Bass has again compiled the subject index. The Editorial Committee deeply appreciates their services. Attention is also called to the fact that it is the Editorial Committee of 1955 (listed on page ii) whose members should receive credit for the selection of the chapter authors of this volume.

J.M.B. J.McV.H.
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PERCEPTION¹

By W. C. H. PRENTICE

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The term perception does not refer to a clearly defined area of scientific study. Instead it points in a general direction and indicates all manner of possible psychological problems and puzzles. May perception be extra-sensory, for instance? May we perceive a pure tone or a single colored patch or a discrimination between two pressures? Is reading words perceiving? Or judging the value of paintings? Or interpreting emotional expressions of human faces? All of these psychological functions get studied by someone or other, but it would be impossible to report on all of them in the confines of this chapter. Arbitrarily, this chapter will deal with visual perception alone, except where other senses happen to be involved with a primarily visual task. It will also limit itself primarily, though not absolutely, to areas in which social and emotional meanings are not involved. Most of the studies here reviewed deal with the visual apprehension of shape, size, distance, color, and the like. Occasionally the perception of the meaning of words will be referred to; less often, the hostility of a face or the purpose of a friend. No hard and fast rules have been promulgated to separate one of these areas from another, and the same investigators often try to extend their concepts of perceptual function from space and object to society and emotion. In general, however, there are reviewed here some of the more traditional attempts to understand man's visual world as it appears in normal life. This is not an exhaustive review. Works on perception in American and European journals have been surveyed. Some have been omitted by design. No doubt others have been missed. It is believed that the sample is unbiased and representative of perceptual research in 1956.

The year 1956 brought very little to the field of perception beyond normal growth. No great discoveries seem to have been made; no unifying insights or novel theories have been propounded. Instead, a rather astonishingly large literature accumulated during the twelve-month period, extending and evaluating old information or old problems. There is still surprisingly little enterprise in what can seriously be called theory. Even those authors who are concerned with systematization and interpretation are traveling in fairly well-worn tracks.

Bruner (13) probably represents the most vigorous attempt at the creation of perceptual theory. In the paper cited, and in other unpublished communications and addresses, he is promoting the idea that perception is a kind of inferential task, a "decision process" as he calls it. Bruner brings new information and fresh thinking to the field, but essentially he is still pro-

¹ The survey of the literature pertaining to this review was completed in April, 1957.

posing an ancient view, namely that complex and ordered sensory experience is primarily a matter of judgment, or guesswork, or betting on the part of the observer, that it is an action process rather than some kind of passive reception.

Vernon (112) presents a closely related point of view, one which shows, through its terminology, its descent from Henry Head. In both Vernon's writing and Bruner's the ancient issues crop up again. Neither makes it clear what kind of assumption is being made about sensory inputs. It would be possible, in a general way, to build a machine that would behave like Bruner's hypothetical organism, but not until we specified the kind of reception and interaction of inputs that will occur prior to categorization. It is equally certain, moreover, that we would have to build into the machine either a prearranged set of categories with differing accessibilities or else build in specifications to determine how such categories and their accessibilities will be created in use. The same holds *pari passu* for Vernon's "schemata." In the absence of specific proposals for these basic steps, the theories seem excessively verbal. It is far from meaningless to propose that perception is an inferential process, but when we know so little about inference, it is small comfort to be given so rough a paradigm. There is a curious situation here. Hochberg (49) has highlighted it in his report of the Cornell symposium. What he calls the "Gestalt revolution" has run head on into counter-revolt. When complex and ordered sensory experience began to show signs of being understood as a function of stimulus variables, optimists in psychology hoped that perceptual research would lead eventually to laws of mental organization that could explain reasoning and inference. Now once again we are being told that we must understand reasoning and inference before we can make sense out of perception. The work of Kohler (60) probably illustrates as well as any the kind of problem posed by contemporary experimental evidence.

The wearing of spectacles that distort the usual physical patterns of stimulation tends in many cases to lead to a kind of adaptation in which long usage results in adjustment not unlike that of normal vision. For example, an optical system that inverts the entire visual field may be worn until everything appears right side up, or lenses that are one color to the left of the visual field and another to the right may be worn until the color differences are no longer apparent. On removal of such glasses, the "real" world is now inverted, or, even more remarkable, looks blue when the eyes turn toward the left and yellow when they turn right. Kohler uses the phrase "conditioned adaptation" for this phenomenon, though without specifying the nature of the conditioning that makes it possible. Clearly no ordinary peripheral adaptation process can account for what happens. The phenomena are conditional upon eye-position, for instance, and that implies the operation of a mechanism that somehow sorts, or categorizes, sensory inputs according to other data. It is easy enough to call these other data cues and to assume that a logical deduction is being carried out by the nervous system,

but no one has determined how that deduction is performed or what its limits are. Some psychologists are skeptical about the ultimate usefulness of the concept of cue with its aura of homunculus. A cue is something to which the actor reacts; in perception, who or what is reacting? What are the laws of reception and organization that determine the nature of cues? What kinds of experience are necessary to establish the effects of particular kinds of cues?

The year 1956 has produced a number of studies that make an attempt to add to our knowledge in these respects. Gibson & Walk (39) were able to show that rats reared in cages containing visually presented circles and triangles (cards mounted in the cage) were better able to learn discriminations between these forms in adulthood. Here is a kind of learning that does not fit any of our usual laboratory paradigms. The mere exposure to the forms may not be enough; with more sophisticated methods of investigation we may discover that the rat must respond in some particular way to the stimuli in question. But so far as present knowledge goes, there is no selective reward or punishment, no differential information, no consistent association of one figure with a different stimulus or response from the other. Only if we assume a kind of automatic visual scanning response that differs for each form can we fit these facts into usual association-theory or conditioning-theory. It is well known that Hebb and his associates propose just such a developmental history for form perception, and Forgas (31) has demonstrated in a very similar experiment the advantage of early experience over that given later. Somehow the early months of life appear to permit more plasticity in these respects than is demonstrable later. It will be of great importance to discover the developmental curve of this kind of plasticity and to find out whether the rate of development is the same for all kinds of perception.

The roles of frequency and of reinforcement in perceptual learning continue to merit careful investigation. A number of studies have added to the store of experimental facts in these areas without really resolving the issues. Warren & Sinha (117) were able to show significant shifts in size preference for cats that had been taught to choose the longer (or shorter) of two lines, but it is not easy to know whether any perceptual change is involved. Sommer (103) and Sommer & Ayllon (104) have attempted to test the influence of rewards and punishments on both tactual and visual discrimination. While seeming to find that reward is more effective than punishment, they too are unable to distinguish clearly between genuine perceptual changes and changes in what the observer reports. This is particularly true in the case of the tactual study.

It is unfortunate that so much time goes into researches that have only applied significance. Much of what passes for perceptual research is only obliquely related to basic processes and requires far more analytic procedures before it can contribute much to theoretical understanding. Two cases in point are the studies of the perception of faces and the perception of words. Adkins (2) and Daston (22) have apparently succeeded in demonstrating

that measures of familiarity in studies of word-recognition have been erroneously based on populations rather than on individuals, but it is not easy to see what light these matters can throw on the general role of past experience in visual organization. Indeed, one may suspect that the preoccupation with the complex tasks of reading and of social perception has led to the view that all perception is a guessing game—as those tasks clearly are.

More traditional problems related to past experience are treated by Slack, who designed an ingenious experiment in which familiar size conflicted with optical size; by Rock & Kremen who re-examined the familiar figural after effect of Rubin; and by McReynolds & Bryan who tried to test an important hypothesis about the acquisition of new percepts.

Slack (98) was able to show that a midget chair whose size was judged in the open at considerable distances tended to be overestimated in height, while an oversized chair was underestimated. The fact that the standard-height control chair was also overestimated raises technical questions and suggests that the results may be contaminated, but in general Slack's main conclusion is probably justified. The experiment should be repeated using a number of other parameters and conditions. In particular it will be important to discover how much experience and what kind are necessary to produce such effects.

Rock & Kremen (92), on grounds of their theory of memory-trace communication, doubt that an ambiguous pattern is more likely to be perceived in a particular one of its two possible organizations because it was so perceived before. They believe that Rubin must have made some error when he reportedly found that figure-ground relationships followed prior appearances. After correcting some possible sources of artifact in the Rubin procedure, they were unable to reproduce this effect. The Rock & Kremen contention could have far-reaching consequences for the theory of perceptual learning, and more information is needed. Since preferences among originally indifferent organizational possibilities undoubtedly do get established, it is essential to discover whether they can be established by mere repetition, and if not what other conditions are necessary.

McReynolds & Bryan (67) asked their subjects to sort some cutout figures in which the subjects had the opportunity to select either unusual and presumably difficult shapes and categories or simpler and more familiar ones. Half the subjects had been allowed to complete another perceptual task while half had had it left unresolved. The authors found, as they had expected, that more novel percepts were sought by those whose prior perceptual problem had been brought to completion. The experiment confounds so many motivational and response variables with the purely perceptual ones that it is difficult to see how it could resolve any issues even if the subjects had been representative. That they were in fact schizophrenic merely compounds confusion. The general hypothesis that one's level of learning about perceptual categories will determine the rate and kind of new acquisitions seems worthy of better experimental planning, and it is to be hoped that some more useful mode of attack will be found.

Apparent movement and the establishment of the postural vertical have also come in for new studies of the role of past experience. Toch & Ittleson (111) have used a variation of a technique previously used by Schiller to show the influence of past experience on the way in which the observer chooses among two or more possible directions of movement in a stroboscopic presentation. Unfortunately, the figures are too poorly chosen and the number of observers is too small to permit much in the way of final conclusion. Solley (102) has demonstrated again the complexity of the kind of learning that takes place in perceptual tasks by showing that his subjects improved in accuracy in returning a tilted chair to the objective vertical in the absence of any knowledge of results. He was unable to discover definitely what cues were involved.

Closely related to these problems is that of recognition of complex figures. Arnoult (4) proposes that it is familiarity that determines recognizability and that familiarity is a negatively accelerated function of frequency. He seems to have demonstrated the latter point by plotting absolute frequency of presentation against subjects' estimates of familiarity. Baker & Feldman (6), in a study that also investigates techniques, demonstrate the feasibility of using threshold luminance measures for recognition in relation to frequency of prior exposure. These background studies are essential to the gradual acquisition of knowledge sufficient to test generalizations at the theoretical level, and Postman & Rosenzweig (87) have added another item in this accumulation by their study of transfer of recognition from one modality to another, a phenomenon that is suggestive of many everyday experiences but is poorly understood in detail. Gaydos (36) has also contributed to this topic in an interesting way.

All of this interest in learning, experience, transfer, and the like is reflected in more discursive and systematic writings. Pastore (82) has renewed the attack on "empiristic" theories of perceiving and posed a number of problems for present-day functionalists. It seems probable that many psychologists have fallen into easy assumptions about perceptual learning because they have felt compelled by methodological considerations to treat perception as a kind of response. Indeed, the idea that perceiving is a mode of responding lies at the base of a great deal of recent writing in this field. Garner, Hake & Eriksen (35) have successfully undermined any notion that such a view is necessitated by operationism or methodological behaviorism. Kanfer (58) in a somewhat less sophisticated fashion has made a similar point, and it would be nice to believe that psychologists will now begin to attend once more to the scientific problems of experience without feeling that they must pretend to believe experience is a kind of myth. Hochberg (50) has added forceful arguments to these concerns though he has been more involved in the divorce of perception from judgment than from response as such. The debasement of the term perception until it meant everything or nothing may be nearly at an end. Until it is, the investigation of alleged influences of past experience can hardly proceed very rapidly or very systematically.

Other studies of the effect of experience and meaning include those of Newton (77), MacBrayer (65), Hall & Treichler (45), and Comalli, Werner & Wapner (20), along with Wiener's monograph on changes that occur in the viewing of an Ames-type room (118).

As one would expect, interest in the roles of personality and of motive has continued, though the number of experimental studies appears to have diminished in recent years. There is less of the polemic demonstration, the "you see, there is something to the New Look," than there was a short time ago. Instead, efforts are directed toward more analysis. Perceptual defense comes in for the major share of investigation. Smock (101) has repeated the Blum experiment that seemed to establish a strong case for defense a few years ago. He contends that Blum's results are attributable to similarities in the figures chosen and that the results cannot be duplicated when the artifact-producing pictures are replaced with others. Lazarus (62), whose own work has been variously criticized for artifacts and alternative explanations, has defended himself vigorously on several counts. Lazarus raises some important theoretical issues. Among the most interesting is the problem of continuous experience which is operated on with discontinuous categories. It is true that certain subception experiments might conceivably demonstrate no more than the fact that limiting the information-content of stimulation makes it inadequate for precise discriminations while leaving it adequate for better-than-chance sorting into crude categories. But Lazarus correctly points out that the situation here described is almost routine in everyday life and that the possibility of motivational intervention is not necessarily ruled out. Lazarus asks for more open-mindedness on interim theoretical constructs and for more empirical investigation. The problem of how the psychologist shall choose his fundamental concepts from among the various data available to him is a topic that is echoed by Hochberg (49, 50) and by Garner, Hake & Eriksen (35). The need for simplicity and communication has led to an overemphasis on concepts directly referable to observable behavior. The need for maximum articulation and representativeness leads in the other direction toward constructs one or two steps removed from laboratory indices. The rapprochement that has been so long in coming will still not arrive until psychologists who wish to emphasize and maintain the richness of mental life are willing to struggle with the problem of definitions that can be tied to empirical observation.

Some more routine items on perceptual defense include Spence's (105) finding that failures to make anagrams out of particular words lead to significant changes in recognition scores, though his data show that experimental words may be affected either positively or negatively, and there is no way to tell when to expect defense and when to expect vigilance. Kissin, Gottesfeld & Dicks (59) have again examined the problem of thresholds for sexually charged words but without adding materially to an already contradictory literature. It is probably time that this technique was put away until the day when we can supply adequate control for interindividual variation, for differences in social atmosphere in the laboratory, and for instruc-

tional variety. We know that all three produce marked discrepancies, but we have inadequate quantitative evidence.

Singer (97) has shown that words giving long reaction times in an association experiment also tend to have higher tachistoscopic thresholds; the obvious hypothesis is that some common factor of anxiety is involved, but he is careful to say that this is still unproven. If the relationship can be shown to be a close one, a variety of uses may be found for an independent measure of whatever inhibitions are tapped by reaction-time experiments.

Greenbaum (42) has presented another study of the role of manifest anxiety in tachistoscopic recognition. High-anxiety subjects tend to have lowered thresholds for hostile faces if they think an evaluation of their work is impending. A prior evaluation seems not to produce any effect, and there are no significant differences for nonhostile faces or for low-anxiety Ss. These limited and complex interactions suggest the need for replication. They also demand better theory than is now available if they do turn out to be repeatable.

In other studies of personal and motivational influences, Atkinson & Walker (5) have sought to find an influence of the affiliation motive on perceptual sensitivity to faces. They presented four pictures, one in each main direction from the fixation point, and the observer was asked to say which stood out most. This measure of salience under brief exposure tended to favor pictures of faces for those subjects above the median in need affiliation as measured by the Thematic Apperception Test. The experimental groups in this study appear to have been combined rather cavalierly, and the degree of association between need and salience of faces is minimal, if indeed it has been demonstrated at all. As other experiments have often shown, this kind of contingency relationship is no substitute for a real function. Again we must wait for careful replication.

Two pairs of authors have found what purport to be relationships between perception and introversion. Two papers by Dureman, Sälde & Johansson (25, 26) give very tenuous evidence for personality influences (introversion and sex) on a measure of reaction to apparent movement that they have devised. Ardis & Fraser (3) report a rather more impressive tendency for introverts to show reduced constancy effects. In general, the totality of evidence seems now to support a belief that statistically reliable associations exist between sex and certain kinds of perceptual attitudes, and, less surely, between those attitudes and more general attributes of personality. Curiously, these attitudinal tendencies do not seem to correlate highly from one perceptual test to another, and we need many more large-scale factorial studies before we will know how much of our present impression can be supported firmly.

A variety of other approaches to the problems of perception as influenced by set, personality, and motivation is provided in papers by Rockett (93), Moffit & Stagner (69), Engler & Freeman (28), Crockett & Meidinger (21), Carpenter, Wiener & Carpenter (15), Wittreich & Radcliffe (121), Taylor (107), Château & Cardo (16), Cohen, Senft & Huston (19), and Bieri &

Blacker (11). Vier (113) has reported a relationship between psychological satiation in the Lewinian sense and critical flicker frequency, CFF dropping with increased satiation. A sociopersonal influence on perception is recorded by Secord, Bevan & Katz (95).

The study of perception as function remains hardly off the ground. The excitement generated only a few years ago by Brunswik, by the Harvard group, and by the disciples of Ames has spread to all the psychological world, but we are still far from being able to say with confidence what may be the perceptual differences between men and women or between psychotics and normals or between children and adults. It is sure to be many years before the assertion that perception is a function of past experience and present need can be more than an instigator to research.

In the meantime, Heron, Doane & Scott (48) have begun exciting studies of perceptual isolation as a means of getting some purchase on the problem of past experience. Their subjects lie immobile and without visual or auditory stimulation for hours at a time. First reports are of profound psychological disturbance involving hallucinations and other sensory symptoms. It is too early to evaluate these McGill studies, but they offer another promising start.

In the field of complex perception, Olum (81) has tried to find out what differences may exist between children and adults in the perception of Michotte's "causality" experiment. It seems clear that the strong tendency to see certain of the "pushing" reactions is not present in the majority of second-grade children. Whether this is due to developmental differences in the perception of stroboscopic movement, as Olum believes, or whether other factors are involved, we cannot now say.

Various other attempts to trace the causes of complex visual phenomena continue in more conventional ways. Piaget and his associates are mining the old field of attention. *La loi de centration*, which deals with overestimation of those parts of the visual field that are fixated, now seems to have been shown by Fraisse, Ehrlich & Vurpillot (32) to be a matter of attention or set rather than fixation, and the "law" is also now being applied to the vertical-horizontal illusion and to the overestimation of curvature in arcs of circles (85, 86). Piaget & Lambercier continue to study age differences in standard perceptual tasks. Recently (84) they have demonstrated that even when the variable rod is near at hand, children are much better at making visual angle matches, being best at about seven or eight years and getting worse until about ten or twelve, then improving once more until about twelve to fourteen when adult responses are achieved. The authors repeat their previous observations of marked overconstancy in adults but note underconstancy in children. Zeigler & Leibowitz (122) have reported parallel results. Their adults show nearly perfect constancy, the smaller children nearly zero. These facts are important for theories of the acquisition of adult perceptual qualities, and more information about what happens between childhood and adulthood to produce these changes is essential. Is this a matter of maturation or of learning proper? If learning, what kinds of rewards or

knowledge of results are involved? How long does it take to produce significant changes? Can we modify those changes experimentally? Pudvitzki (88) has found a loss of constancy in older subjects, but it is not clear how his data fit the general picture.

A quite different type of age change has been investigated by Basowitz & Korchin (8) who find that young adults are far better at completing Street-Gestalt figures and at finding hidden parts in Thurstone's modifications of Gottschaldt's test than are older (median age 78) people, who show marked rigidity and perseveration. Wallace (115), using a more complicated experimental arrangement and more different kinds of materials, also found a progressive deficit for older people, mainly in their inability to integrate or analyze as quickly as younger viewers. In general, the deficit of the aged could be compensated for by increased viewing time. Wallace also reports some tendency for specific categories of response to be fixed in the older subjects, and one wonders how much of that kind of stereotyping may be related to language habits. An old hypothesis is frequently mentioned by psychologists but rarely tested, namely that perception is aided by verbal labels. Numerous writers have quoted this supposition as gospel, but no adequate test had ever been made until the paper of Hake & Eriksen (44) who carefully designed an experiment that would differentiate between improvement in the use of the labels and improvement in recognition of forms so labeled. It is now clear that recognition as such is not enhanced, and a persistent psychological myth can probably be laid to rest.

Returning for a moment to the persistent problems of size and distance, it is only possible in the space at our disposal to mention a few of the many studies that continue to deal with that complex subject. It is appalling how little we really understand about space perception after centuries of thought and decades of unremitting experimental investigation. Clark and his associates have been studying the perception of slant, a key aspect of Gibson's theory of visual space (17, 18, 43, 99). Gogel (40, 41) has published two monographs on the perception of distance as a function of direction and of lateral separation. Hastorf & Kennedy (46) have made an attempt to clarify the relation between Emmert's Law and size constancy (though it hardly seems to have needed clarification). Holzman & Klein (52) and Gardner, Holzman & Siegal (34) find that such nonvisual influences as weight and serial order of presentation may affect size judgments in various kinds of experimental arrangement. Leibowitz (63) has partly clarified the applicability of Thouless and Brunswik ratios in the constancy problem. Mukerji (75) has been studying the ability of rats to make comparative height judgments, and Quaranta & Summo (89) have followed the development of depth perception in ducks.

Shape constancy, that twin of size constancy, comes in for another look by Leibowitz & Bourne (64); while Künnapas (61), like the Geneva group, is still trying to make sense out of the vertical-horizontal illusion, attributing it now to the oval shape of the visual field and demonstrating that it can be modified by changing the shape of the surround.

Squires (106) has contributed an important item in his demonstration that Lau was correct when in 1922 he contended that stereopsis may be produced without horizontally disparate stimuli. This is a persistent psychological problem, and many writers have contended that anatomical disparity as such could not explain the complex data of stereoscopic vision. If we can understand the phenomenal disparities that are necessary to produce depth, we may be better able to take account of the conflicting evidence about retinal disparity. Not unrelated, though leaning heavily on meaning rather than on shape or direction as such, is a study by Engel (27) on the role of content in the resolution of binocularly presented materials. Engel's purpose was not directly related to the problem of stereopsis, but his work suggests that rivalry and fusional processes are in part a matter of order based on past experience.

The most exciting studies of the year in the field of stereoscopic vision appear to be those of Motokawa, Nakagawa & Kohata (73, 74). Using Motokawa's well-known but little-understood technique of retinal induction, they have found a decrease in the difference between electrical thresholds following white light and those following successive red and white light wherever nearer objects or object-parts are located in the visual field and an increase of this same difference wherever farther parts of the field are located. The same findings are repeated with monocular depth (74), with retinal changes being correlated with experience of distance in geometrical illusions and in figural aftereffects. It is too soon to assess the physiological meaning of these findings, but at minimum they indicate a peripheral electrochemical change associated with pattern organization. Everything we know about visual neurology suggests that this must be a secondary result of efferent activity; certainly in the binocular cases, that is an inevitable conclusion. The establishment of such effects raises all kinds of possibilities for limitation of input by extant processes, possibilities that may make physiological sense some day even out of perceptual defense.

Another aspect of three-dimensional perception turns up in the revival of interest in the stereokinetic effect. Wallach, Weisz & Adams, whose work brought this old problem back to our attention, have continued Wallach's analyses (116); Fischer (29) has studied certain variables affecting the phenomenon; and Miles & Birand (68) have shown how it is affected by surface lightness and contrast. Gerathewohl & Cibis (37) report a new effect of lightness on depth that is independent of rotation.

Visually perceived movement is receiving very little systematic study, but various aspects of it have led to published comments during this year. Rattleff (90), attempting to get at the form-color differentiation among viewers, used a technique that permitted identification in perceived motion to follow either a form or a color dimension. Apparently very slight color or brightness differences will permit form to dominate the experience. Johansson (57), who has long been interested in the aftereffects of movement, finds that the velocity of these aftereffects is independent of the velocity of the originally perceived movements. Gibson *et al.* (38) compared the accuracy

with which motion may be perceived during fixation and during pursuit; they found no differences. Smith & Sherlock (100) have taken up the issue of velocity-transposition and present a frequency theory for the Brown experiment. It is hard to see how this view can account for ordinary speed constancy which otherwise seems to behave like transposition. Other studies of movement that are worth noting include those of Mulholland (76), of Toch (110), and of Morin, Grant & Nystrom (72). Humphrey (53) has made the curious proposal that the Phi-phenomenon be considered a form of conditioned reflex. He notes certain formal properties in common (the dependence of a reaction on prior stimulation, etc.) but fails to make a convincing case. Nevertheless the possibility that the basic neurological interaction between separate stimuli might be the same in both cases is an interesting one. Certain aspects of S-S conditioning might be clarified by such a concept, and it bears more thorough investigation.

Form as such seems temporarily to be almost a dead issue. The Gestalt laws of organization have proved unwieldy, and it is certainly time for another attempt to do something more systematic. Dinnerstein & Wertheimer (24) have attempted to go beyond the suggestions of Ratoosh in dealing with phenomenal overlapping, but we are still without a satisfactory set of functional generalizations. Brand & Cohen (12) have tried (unsuccessfully, it would seem) to give an informational analysis of figural goodness in relation to recognizability. Hochberg & Silverstein (51) have struggled with the problem of objective specification of stimulus similarity; may they continue their efforts! Other miscellany in this area include Johansen's phenomenological study (56) of three-dimensional form and Pheiffer, Eure & Hamilton's reappraisal (83) of the role of eye-movements in the organization of reversible figures.

Perception as a function of retinal locus and orientation was a favorite problem a few years ago. Two new investigations bear on those issues. Rock (91) has tried to demonstrate that orientation may be referred to either environmental or anatomical standards and that both kinds of orientation may be effective in recognition. Heron (47) has studied the problem of orientation from the point of view of expectation as well as experience; he finds that English words are more readily recognized in the right half of the visual field, but that recognition in the left-hand half can be materially improved by warning the subject that that is where the words will appear.

Several students have dealt with a variety of problems involving closure. Mooney, in particular, has dealt with objective influences in two papers (70, 71). Hunter & Duthie (54) have been concerned with the influence of interpolated stimuli, and Walker & Veroff (114) have reported a new experiment in which the technique of successive reproduction is compared to mere lapsed time. Basowitz & Korchin (8) treat closure in relation to age.

Intersensory phenomena have been largely neglected in 1956. Two items of interest concern the studies of Ogilvie (79, 80) on interactions between CFF and auditory flutter and those of Jaffe (55) on visual stimulation as an influence on kinesthetic figural aftereffects. Neither of these is an entirely

new discovery, but both confirm a long known and little understood aspect of perceiving, one that has received too little attention in recent years. The cross-modal qualitative similarities that interested Hornbostel and other early Gestalt writers offer a fertile field for the study of organization; that field should be tilled more assiduously.

Various aspects of psychopathology and neuropathology have, as usual, attracted a share of attention. Williams *et al.* (120) found that brain-injured do, as previously reported, tend to rotate copies of designs significantly more than normals, but the differences are slight. More striking is the finding that normals apparently make greater use of peripheral cues and that both normals and injured are strongly influenced by shape and orientation of surround. Battersby *et al.* (9) studied autokinesis in a variety of neurological patients. They find that interactions between autokinetic experience and rotation go beyond what can be attributed to eye-movements, thus supporting some kind of sensory-tonic interaction; subjects with visual impairment arising from brain damage tend to see movement toward the normal side, but this direction can be reversed by rotation except in somato-sensory and motor injury. Baldwin, Lewis & Frost (7) experimented with the striking hallucinatory effects of ingesting LSD-25 and of isolation in darkness. In man these effects are clearly reported as sensory experiences. Chimpanzees show behavior that leads naturally to the inference that they are seeing things. Temporal lobectomy destroys the hallucinatory influence both of lysergic acid and of prolonged darkness. Niebuhr & Cohen (78) compared normals, neurological patients, and schizophrenics (both acute and chronic) on a number of visual tasks. The schizophrenics do progressively worse than normals but better than the brain injured on the Bender-Gestalt test. Also note Cohen, Senf & Huston (19).

Studies of discrimination, threshold-functions and related psychophysical problems do not normally get included in what is known as perception. A few characteristic investigations may be cited, however, to indicate the nature of a current borderland between what have classically been considered sensation and perception respectively. Ryan & Schwartz (94) have studied the influence of various modes in which visual forms may be presented and have shown substantial effects of difference in complexity on recognition-speed. Since these differences do not seem to follow directly from the degrees of complexity involved, they are especially worthy of notice and further investigation. Adams (1) has published a study of accuracy of detection of low intensity stimuli, showing that both brightness and duration are involved and that accuracy declines over a 110-min. work period but recovers almost completely in a 10-min. rest period. Battig, Voss & Brogden (10) report their finding that the apparent brightness (i.e., luminance of a matching steady light) of a flickering light decreases with increase of frequency of intermittance from 4 cps to fusion. Foley (30) has studied the effect of background on CFF and reports that it increases directly with size of background. His technique seems to have assured that this effect is not due to border-influences. Thomas (109), studying the Sherrington effect in CFF, found

it enhanced by the addition of contours to the stimulus-patches presented to the two eyes; contrary to his expectations, congruent patterns increased the effect no less than conflicting ones. Della Valle, Andrews & Ross (23) report on thresholds for the perception of curvilinearity and of angularity, comparing the two kinds of inhomogeneity and recording the influence of chord and base lengths. Shaw's new theory of color vision (96) probably does not belong in perception at all, but it is a stimulating effort that can hardly be overlooked by anyone with an interest in visual phenomena, and it does take more account of such perceptual phenomena as contrast than many such theories.

The dearth of systematic writing in the field of perception has already been noted, but the reader's attention should be called to a few items that contribute either to method or to some aspect of theory. Gaffron (33) believes that we need a new look at visual phenomenology, and she argues cogently for a number of new dimensions of visual experience, dimensions that she was led to examine in trying to understand changes in the appearance of complex patterns when inverted right for left. McCurdy (66) has applied the concept of schemata to studies of the influence of value on apparent size; he comes to the conclusion that available data may be accounted for without assuming any truly perceptual influences, as distinguished from memorial ones. Taylor & Papert (108) present a theory of perceptual constancy; Wilcott (119) shows how subliminal stimulation may mean anything or nothing until we agree on definitions about threshold determination; and Campbell (14) discusses the possibility that visual perception permits a kind of trial-at-a-distance that guides responses of visual animals on the basis of previously learned object-consistent reactions. All these attempts to illuminate methodological and systematic issues are rewarding. They are no substitute for empirical investigation, but each in its own way is capable of generating productive experimental studies that should push us on our way to understanding how it is that animals perceive at all and why they perceive what they do.

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VISION¹

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Since the topic of Perception is separately covered in this volume (in the Chapter by W. C. H. Prentice) the present chapter is intended to emphasize the psychophysical and physiological approaches to the topic of vision. Because of restrictions on length, the coverage of this review is limited to papers appearing during the year ending in April, 1957. Necessarily excluded are military reports, papers primarily of ophthalmological interest, and research in which visual stimulation is incidental to studies of learning, motor responses, or other topics.

PHOTORECEPTORS

Rushton (130) has shown that a method developed by Campbell and himself can be used to obtain significant quantitative information on the absorption of light by photoreceptors in the living human eye. Lights of various wavelengths are directed through the pupil to the fundus of the eye, and the light returning (about .00005 times the incident light) is carefully measured. In one series of experiments, a comparison is made between measurements on the dark-adapted eye and measurements on an eye that has been bleached for 10 minutes with white light at high intensity. The result is a difference spectrum for retinal absorption, a function that resembles the absorption spectrum of rhodopsin, but is displaced 5 to 10 $m\mu$ toward the red.

Not content with these results on the relative spectral sensitivity of the retina, Rushton (131) has gone on to explore the question of the absolute optical density of visual pigments as they are found in the rod receptors. This matter is indeed a complex one. Assumptions or estimates had to be made with regard to such factors as stray light and the distribution of the rhodopsin over the retina. Using the best data currently available it is calculated that about 30 per cent of light reaching the rod receptors is absorbed. Since rods occupy about 70 per cent of the area of the retina involved in these experiments (a region 20° out from the fovea) it is concluded that about 20 per cent of the light reaching the retina at this point is actually effective in stimulating rod receptors. Assuming a 50 per cent loss in light transmission through the media of the eye this means in turn that 10 per cent of the stimulus light is effective. For green light of maximal effectiveness in the dark-adapted eye this confirms the estimate of Hecht that from 5 to 14 quanta of light are the minimum necessary for visual stimulation.

¹ In this chapter the following abbreviations will be used: CFF (critical flicker frequency) and ERG (electroretinogram).

² Preparation of this review was aided by a contract between Brown University and the U. S. Office of Naval Research.

Rushton's value of 30 per cent absorption by the human retinal rods is equivalent to an optical density of 0.15 log unit. This value is high by comparison with most values of rhodopsin found by methods of extraction from the eyes of animals. Lower values have led others to adopt single-quantum or two-quantum hypotheses for the visual threshold, hypotheses that present great difficulties in relation to functions for the frequency of seeing individual flashes. The higher density values are more credible if one further assumes, as Rushton does, that a light-trapping mechanism exists whereby light is internally reflected down the rod receptor. Small amounts of rhodopsin are thus effective in absorbing the light so concentrated, and the present optical measurements of density are therefore deemed to be more indicative than are the chemical ones of the number of quanta necessary to stimulate the eye.

Rushton (132) has more recently begun to study the presumed cone pigments in the living human eye. The method is one of selective bleaching by filtered lights of various colors. Results are in agreement with curves derived by Stiles for red- and green-sensitive receptor systems. They are also consistent with a mechanism of light-trapping within the cone receptors.

Hagins (68) has used a similar technique to study the immediate effects of light in excised rabbit eyes. He finds a measurable fading response whose time course is half complete within .001 sec. This response may well represent Wald's process of conversion of metarhodopsin to retinene-protein. It represents a response much more rapid than the electroretinogram or any other known physiological event elicited by light. Evidence for light-trapping is found here, and also in a study by Tansley & Johnson (155) of cones in the grass snake.

Peskin (117) reports an optical density of 0.642 in the outer segment of retinal rods in the frog. This high value is obtained by extracting rhodopsin from rod outer segments, estimating their number, measuring the density of solution, and referring this to a volume equal to that of the rod segments involved. He thus estimates an 80 per cent absorption of light reaching the receptors.

Denton & Warren (46) and Straus (133) have reported visual pigments in deep-sea fish that are well adapted to receive the blue-green light that reaches them in their natural habitat. These pigments, which are yellow, have been called chrysopsins or visual golds. Munz (110) has found that the photosensitive pigment in a teleost fish is neither rhodopsin nor porphyropsin. Crescitelli (41) has described a visual pigment in lizards that resembles porphyropsin in color but has other properties connecting it with the retinene₂ system. The spectral absorption curve for this pigment is in close agreement with a spectral response function for the lizard that Denton (45) has determined by the use of the pupillary response. Apparently the classifications presently used are unable to cope with the visual pigments described in these studies.

Visual pigments are further described in papers by Albrecht (3), Bridges

(22, 23), Dartnall (42), Hubbard (81), and Radding & Wald (123). The studies of Wald & Burg (167) and Wald & Brown (166) have revealed that in lobsters and certain crustacea vitamin A plays a role similar to that which they have described in detail for vertebrate eyes.

An exciting new development in the study of photoreceptors is the use of the electron microscope to reveal the structural details. Wolken (176, 177) has embarked on a comparative study of plant chloroplasts, *Drosophila* eye pigments, frog retinal rods, and cattle retinal rods. In the case of the visual rods, he has confirmed the existence of the submicroscopic striations described by Sjöstrand. Using new thin-section electron micrographs Wolken has found many points of similarity in the detailed structures that are sensitive to light in plants and animals. He has developed a model for the molecular structure of the retinal rod receptor. This model describes the outer cylinder of the rod as composed of alternate discs of lipid and aqueous protein complexes separated by monomolecular layers of pigment molecules. Such a model will certainly serve to stimulate further research on the structure and function of photoreceptors. The possibility of uncovering common modes of action in photosynthesis and in vision is a challenge indeed.

PUPILLARY RESPONSE

A comprehensive study of the human pupillary response is described in a monograph by Petersen (118). The method is one of simultaneous motion picture photography of the two pupils, using infrared light. Extensive data are reported on the time course of constriction and dilatation under various conditions of light stimulation. The study also includes the effects of age, drugs, anoxia, and various pathological conditions. Schweitzer & Bouman (137) have measured pupillary responses at various light levels, finding that at low levels the response is clearly mediated by rods and at high levels by cones.

The relative contributions of pupillary constriction and other factors to discomfort produced by a glare source are evaluated in papers by Fry (58), Fugate & Fry (59), and Hopkinson (77). Despite clearly demonstrated correlations, the causal relations were not simple ones. Discomfort is not caused primarily by pupillary constriction, nor is constriction primarily the result of discomfort.

Weale (170) has found that frogs (but not toads) reliably exhibit direct pupillary contraction when light falls on the iris. This nonretinal response may be a relic of dermal responses to light among lower vertebrates, and is presumably not present in higher forms.

BRIGHTNESS DISCRIMINATION

Herrick (75) has studied the effect of flash duration in a luminance discrimination task involving a 1° foveal test patch. Increments and decrements were found to be equally well discriminated over a wide range of adapting luminances and exposure durations. An equation derived by Graham and

Kemp from an earlier formulation by Hecht was found to describe the data, even though some of Hecht's underlying assumptions have been found to be in error.

Hunt (82) has argued that the threshold value of $\Delta I/I$ for any retinal area should be proportional to $f^{-1/2}$, where f is the number of nerve fibers serving that area. He uses acuity data to estimate number of functional fibers, and gets confirmation for his hypothesis over a range of spot sizes from $10'$ to $40'$ in diameter.

Hake & Averbach (69) found that foveal brightness discrimination is enhanced when the two fields to be compared are joined by a transition zone in which luminance increased linearly from the value presented in the darker field to that presented in the lighter. Separation of the two fields by a dark line, on the other hand, is unfavorable for discrimination of the two brightnesses. It is concluded that the region between the fields is a critical one and that the presence of the linear gradient facilitates mechanisms of sharpening that result from spatial interaction.

Vos, Lazet & Bouman (164) measured contrast thresholds by the use of small square targets and Landolt ring targets. Somewhat higher thresholds were found than those reported by Blackwell, and methodological differences are assumed to be the explanation. Fairly large individual differences were found among the observers, but practiced and impracticed observers were not found to be significantly different.

VISUAL ACUITY

A detailed consideration of theories of visual acuity is given by Falk (56). He first reviews the evidence earlier put forth by Weymouth, Wilcox and Purdy, Senders and others to show that the relation of the retinal mosaic to the geometrical image is not a sufficient basis for analyzing the problem of acuity. Falk points to some recent eye movement studies that reveal good acuity under conditions of minimal movement. He asserts, however, that eye movements are always an aid to acuity in normal vision. The present reviewer believes that this last statement is too extreme, and that eye movements, while necessary for prolonged viewing, have not yet been shown to improve visual acuity as usually tested. Falk's review properly emphasizes the need for more careful consideration of neural factors at the level of the retina itself.

Pirenne (121) explicitly considers retinal factors when he discusses the possible responses of "off" units to the black elements of acuity test objects. These units may respond to cessation of illumination on presentation of the black object or on the occurrence of saccadic movements that serve to bring the black image to fresh regions of the retina.

Day (43, 44) is primarily concerned with a statistical analysis of form perception. The title of his second article is somewhat misleading in that his main concern is not with retinal factors but with hypothetical central ones having to do with the perception of form.

Brown, Kuhns & Adler (30) have measured acuity in the dark-adapted eye. The results show that, for the resolution of fine detail, approximately the same luminance is required for all the colors used. Coarser and coarser gratings require progressively less luminance for all colors, but the change is much less for red light than for blue, with other colors occupying intermediate positions. These results indicate the shift from cone function at high luminance levels to rod function at low, despite the fact that the eye was dark-adapted throughout. The practical consequences of this are pointed out for night flying and other situations involving thorough dark adaptation with momentary viewing of displays requiring good visual acuity.

Herrick *et al.* (76) have obtained data on the detection of a separation between target spots on a simulated plan position indicator radar scope. Among the many significant factors, background luminance was found to have the greatest influence on the acuity scores.

Berger (14) reports a steady decline in acuity during one-hour periods of testing. This he attributes to fatigue in the accommodative mechanism. Reading, however, was found to have no such clear effect on acuity scores. Rest periods of five minutes serve to restore acuity that has been lost through fatigue. It would seem to this reviewer that central factors may have been of major importance during the hour-long testing sessions in which Landolt C targets were being judged. Indeed Adams (1) has reported a significant "lapsing of vigilance" over a detection testing interval of 110 minutes with Air Force cadets.

Sloan (140) has called attention to the fact that ultraviolet light may degrade the contrast between target and background and thus yield poor scores on the Harrington-Flocks screening test. This is particularly true of aphakic patients, by whom ultraviolet light is readily seen.

Sutherland (145) reports that even an octopus exhibits better vision for horizontal or vertical test objects than for those of oblique orientation. He proposes a neural model in which the excitation patterns are arranged in columns and rows to mediate the height and lateral extent of the target figure. Some references to the optics of the eye and the nature of photo-receptors are in a report by Brown (28).

BINOCULAR VISION

Gillott (62) describes the research being done on aniseikonia in the Department of Applied Optics at Manchester (England) College of Technology. One aim is to determine the extent of differences in the sizes of the right and left ocular images in a normal cross section of the population. Tables of results are given for 100 subjects examined with a space eikonometer. Of these, 42 per cent exhibited size differences greater than 0.8 per cent and 7 per cent showed differences greater than 3 per cent. All of the latter group showed symptoms of visual strain.

Bouman (20) has measured contrast and increment thresholds for one eye under conditions in which various adapting stimuli were provided for

the other eye. While the steady thresholds were not influenced by other-eye adapting conditions, various fluctuations did occur from moment to moment in the effectiveness of the test of flash, especially when intermittent illumination was supplied to the other eye. Results are accounted for in terms of central attentional or rivalry phenomena.

Forbes & Mote (57) made a study of binocular and monocular threshold measurements during the course of dark adaptation. Using an 8° test field they did not find significantly lower absolute thresholds for binocular stimulation as compared with monocular, nor was variability significantly smaller in the binocular case.

Rønne (129) has advanced the hypothesis that binocular rivalry or suppression is the key to stereoscopic depth perception. Thus rivalry is not present during fusion of two similar images of a given target. Disparity of images results in more and more rivalry, however, with attendant judgments of "near" or "far" and a tendency toward changing the convergence of the eyes in a direction to reduce the disparity. Ogle (114) presents a new evaluation of convergence as a cue for stereoscopic acuity. His method is to require the subject to adjust two test objects to an apparent equality of distance from him. In one condition the subject is allowed to look from one target to the other while making the adjustment; in the other the subject must make his settings without moving his eyes. The result is a superiority of the eye-movement method, but the extent of such superiority can best be accounted for on the basis of using a more favorable location on the retina for the judgments to be made. On this and other evidence it is concluded that movements of convergence are not important cues for stereoscopic depth. Squires (143) has confirmed the earlier experiments of Lau that demonstrate stereopsis due to visual illusions. Thus an apparent disparity would seem to suffice for depth judgments in the absence of real disparity in the critical figures for the two eyes.

Westheimer (172) shows that, in the case of optical aids for observing near objects, the angle of convergence has significant stereoscopic effects. Thus magnification should not be the sole criterion for selecting binocular loupes or similar devices. The judicious selection of a prism and lens combination may yield better depth judgments than those obtained with magnifiers alone. Sloan & Habel (141) have worked out the basis for specifying an appropriate magnification to be used with subnormal vision. This treatment is based not only on the visual acuity of the patient but also on the closest distance for which he can accommodate without the magnifiers.

New arguments for regarding the human eye as a non-Euclidean instrument have been advanced by von Schelling (162). He cites as examples certain modified forms of Luneberg's binocular visual space and of the chromatic space that has been developed to describe differences in chromaticity.

For stereopsis as measured by the Howard-Dolman two-rod test, Micko (99) has found that angular disparity thresholds are not significantly related

to absolute viewing distance, over a range from 25 to 200 meters. Depth judgments are found to be influenced by brightness and hue in the experiments of Mount *et al.* (108). The mere occurrence of hue is sufficient to cause the object to appear nearer than its achromatic background regardless of the specific hue that is used. Contrast with background yields apparent nearness, especially when primary determiners of depth are not present.

DARK ADAPTATION

Blough (17) has given a full account of his pigeon experiments using the ingenious method developed by Ratliff and himself for animal psychophysics. The curves of dark adaptation exhibit the traditional cone and rod portions but the cone portion is found to be relatively more prominent and longer lasting than for the human eye.

Boynton & Kandel (21) have devised various sequences of adapting stimuli that have the effect of raising or lowering the threshold for a test flash. The results indicate that the sensitivity of the eye is importantly determined by the neural "masking" effects produced by responses to any conditioning stimulus. The masking effects often predominate over the photochemical effects, completely blocking the transmission of impulses that normally would follow the presentation of the test flash.

Baker, Debons & Morris (10) have found that the course of dark adaptation is determined chiefly by the total amount of light presented to the eye in the preadapting field. Mote & Forbes (103) have measured the course of dark adaptation following preadaptation to steadily increasing or decreasing luminances of adapting field. Higher initial thresholds and longer times for the attainment of complete dark adaptation were found when pre-exposure was from zero to maximum intensity rather than the reverse.

Pirenne, Marriott & O'Doherty (122) have published the results of World War II experiments on luminance levels for seeing Landolt rings, and Hartline & McDonald have contributed an appendix to that report in which they outline methods for testing vision in the dark-adapted eye.

In a study of various neurotic subjects Granger (64) has found that in dark adaptation the final thresholds attained by hysterics are lower than those of anxiety patients. Zeavin & Wald (180) have made perimetric studies of dark adaptation in patients with a progressive degenerative disease of the retina. They found in most cases that rod vision had deteriorated early and cone vision later. This is consistent with the hypothesis that retinitis pigmentosa involves an inability of the receptors to use vitamin A, for it is known that synthesis of pigment is more rapid for cones than for rods.

Brown (29) suggests the use of a cone-to-rod ratio as a specification for lighting systems that are to be used by dark-adapted pilots. The advantage of red light is seen to diminish as progressively lower illuminations are provided. Richards (125) has compiled a review of recent research on night road illumination and related visual problems.

Hopkinson (78, 79) has been concerned with scaling brightness, and Weale (171) has raised a number of perplexing problems in connection with peripheral vision.

VISUAL THRESHOLDS

The nature of the threshold for vision continues to stimulate experiments and discussion. Barlow (11) has considered the hypothesis that events occur in the retina or elsewhere which cannot be distinguished from the events resulting from the absorption of quanta of light within the retinal rods. He points out that irregular and apparently spontaneous impulses are found in the optic nerve of the cat; that this necessitates a signal-to-noise ratio represented by the coincident absorption of several light quanta for vision as Hecht, Schlaer and Pirenne have estimated for the human eye; and that a subject who is "guessing" to the extent of giving even a small percentage of false positive responses to light thereby yields a frequency-of-seeing curve based considerably on responses to "noise." Marriott (98) has noted that similar conclusions may be derived from a paper by Pirenne and himself on the frequency-of-seeing curve.

Howarth & Bulmer (80) are also concerned with the probabilities of positive and negative responses in threshold experiments. Their work, however, is concerned with incremental thresholds for added patches of light in the light-adapted eye. It may be concluded that often the apparent fluctuations in threshold are the result of sequential response dependencies rather than fluctuations in stimulus or sensory events.

FLICKER

Ettlinger (55) has compared critical frequencies of fusion (CFF) for flicker in central and peripheral fields. Wide individual differences are found. With bright surrounds, about equal proportions of subjects had a higher CFF in the center than in the periphery; with dark surrounds, most subjects had a higher central than peripheral CFF. Brown (31) has described a simple demonstration of the difference between center and periphery, stating that the central CFF is lower than the peripheral under the conditions of the demonstration.

Mahneke (96) has found that the value of the obtained CFF is dependent on the value of acceleration or deceleration used in the test of flicker. Ogilvie (112, 113) has demonstrated a dependence of visual CFF on the simultaneous presentation of auditory flutter having the same frequency. The CFF is higher when the flutter is in phase than when it is out of phase. This is true for three levels of luminance for the flickering light.

Landis & Hammi (92) have found no correlation between CFF and intelligence, using various groups of psychiatric patients as subjects. They feel that correlations reported by others were the result of random fluctuation in the determiners of CFF.

Thomas (158) has compared CFF values for in-phase and out-of-phase

presentation of flicker to the two eyes. Having confirmed the higher CFF reported by others for in-phase stimulation, he developed the hypothesis that the effect is attributable to the existence of brain cells that receive excitation from corresponding areas in the homonymous hemiretina. It was predicted that the phase difference would be accentuated by the presence of added contours in the visual field, and this was indeed found to be true.

Gerathewohl (61) has found that with a low contrast, conspicuity increases with flash frequency; with low frequencies, conspicuity increases as brightness contrast increases. A signal flashing three times per second is highly effective when at least twice as bright as its background.

Smythies (142) has given an introspective account of the visual phenomena produced by flicker of a large, uniform field. Three alternative neural hypotheses are offered to account for the results, and it is suggested that the subjective phenomena be used in connection with personality tests. Walter (169) has observed the subjective colors that are seen with white light flickering at a rate of 5 to 10 flashes per second. The significance of the color effect is attested by the fact that even normal persons give deuteranopic and protanopic responses to the Ishihara test plates when they are viewed under flickering illumination.

Two papers by Baumgardt and LeGrand (13, 93) have been concerned with visual fatigue as caused by the flicker of fluorescent lamps. Lamps operated on DC are the controls. A slight difference in favor of the steady light source is found after four hours of intensive visual testing, the criterion being range of accommodation. Collins (38) has observed that some fluorescent lamps produce a 50-cycle subharmonic in addition to the 100-cycle flicker produced normally with the European 50-cycle alternating current. The subharmonic, even in small amounts, yields discomfort. Helson, Judd & Wilson (72) provide equations and tables for predicting color with fluorescent as compared to incandescent illumination.

Battig, Voss & Brogden (12) have found that the apparent brightness of a flickering light shows a decrease as flicker rate rises from 4 c.p.s. to the CFF, using low and moderate levels of brightness. Thus no evidence for brightness enhancement was found here. Buchmann-Olsen and Rosenfalck (33) have calibrated the glow-modulator tube that is ideal as a light source for some flicker experiments.

ELECTRICAL STIMULATION

It has long been known that visual "phosphenes" can be elicited by passing electric currents through the retina by the use of skin electrodes located near the eyeball. Gebhard *et al.* (60) have accomplished the technically difficult feat of getting difference limens for frequency discrimination with visual phosphenes of constant intensity. The resulting values of $\Delta F/F$ lie between .009 and .03, values considerably lower than those obtained by earlier investigators using less stable equipment. Nevertheless these values are somewhat higher than those for comparable photic flicker data, especially

at flicker rates above 30 c.p.s. The electrical data show a minimum at about 25 c.p.s. for the difference limen, while the photic data show a maximum in this region. The presumption is that the difference lies in the fact that photochemical reactions are by-passed when the eye is stimulated electrically.

The series of electrical stimulation papers by Motokawa and his school has continued. This year they report (2) confirmation of the absence of rods in the fovea and the zone at which parafoveal rods begin to appear according to Osterberg's histological data; (87) confirmation of earlier results on velocity of spread of "retinal induction"; (90) practice effects in observing flickering phosphenes; (102) the possibility of selectively stimulating receptor cells with low frequencies, and bipolar cells with higher frequencies of alternating current; (106) the superior stimulating power of flickering light at 10 to 20 c.p.s. as compared to steady light, in enhancing phosphenes; (107) the effect of 0.1 to 0.4 r stimulation of the human body by x-rays, as judged by increases in flicker phosphene response; (134) the relation of the flicker phosphene response to the time constants of stimulation of retinal tissues in light and dark adaptation; (146) confirmation of the Ferry-Porter law, with separate rod and cone effects, when flickering light was used to enhance electrical phosphenes; (115) a lack of the specific red response in a protanomalous subject; (116) a detailed correspondence between the electrical curve of "retinal induction" and psychophysical curves of colorimetric purity; (147) effects of blood transfusions on electrical flicker thresholds; (104) the differential phosphene effects resulting from such depth cues as interposition of objects, linear perspective, figural aftereffects and ambiguous figures; (105) corresponding effects with stereoscopic depth situations; and (91) long-persistent aftereffects resulting from the presentation of supra-threshold electrical test stimuli. There appears to be no limit to the range of topics to which the electric phosphene method is applied. In general the results reported are in remarkably close agreement with information already obtained by psychophysical methods.

A monograph by Riggs, Cornsweet & Lewis (127) describes a series of investigations attempting to confirm the original experiments of Motokawa whose aim was to generate the fundamental response curves for the color receptors. These experiments were designed to determine the time course of the aftereffects of colored lights on the thresholds for electrical stimulation of the eye. In the present experiments, precautions were taken to minimize the effects of changes in electrode and skin resistance, timing was automatically controlled, and all thresholds were determined by the use of psychophysical procedures in which the stimulating electric current was varied by equal log steps. Negative results with respect to color were found in all subjects. Some subjects showed electric threshold changes affected by intensity of light, but others showed no consistent effects of this kind. The authors conclude that Motokawa's positive findings on color, and also the phenomenally low variability of all his data, result from his use of a somewhat elastic psychophysical procedure. Motokawa had found each electric

phosphene threshold by starting with a moderately high intensity, skipping down by large steps to low intensities where he expected to find the threshold, and massing the judgments in this region. This procedure would seem to increase the probability of finding a threshold within a preconceived and narrowly restricted range of intensities. Motokawa (91) believes his procedure to be necessary because he feels that suprathreshold stimuli have long-lasting deleterious effects. Riggs, Cornsweet & Lewis (127) on the other hand, have evidence that no such effects are occurring; and believe that these effects have been eliminated in their own experiments, by their precautions to minimize the effects of skin and electrode resistance changes. They do not endorse the method of electrical stimulation for the study of color vision.

COLOR VISION

An encyclopedic review of theories of color vision has been done by Müller-Limmroth (109). He cites modern evidence from optic nerve and electroretinogram recording, together with new photochemical evidence and the evidence provided by psychophysical experiments. The review is enumerative rather than critical, but the author does conclude in favor of a theory that takes into account the responses of the retinal rods as well as cones to lights of various colors. He also outlines the CIE, Ostwald and Munsell notations for specifying color. An extensive bibliography is not included with the published article but may be obtained from the author on request.

Hurvich and Jameson have continued their quantitative treatment of a three-variable, bimodal opponent-colors theory of the Hering type. The current papers are concerned with the effects of chromatic adaptation on colors (84); the numerical specification of colors in terms of hue, brightness, and saturation (83); and an analysis, in terms of the theory, of the color discriminations of protanomalous and deuteranomalous observers (85). This analysis is based on the assumption of two peculiarities of the anomalous trichromats. First, they are assumed to have undergone a small shift in the set of photosensitive distribution functions along the wavelength scale. The protanomalous functions are shifted toward the short-wave end of the spectrum, while the deuteranomalous are shifted toward the long-wave end. Second, the red-green chromatic response function is assumed to have suffered an impairment or loss. With these two assumed changes these authors have made an impressively successful set of theoretical curves to fit the existing data on anomalous trichromasy. A major test of the theory will, of course, result from the direct evidence that may be contributed in the future by biochemical or electrophysiological studies of the cone photoreceptors.

MacAdam (94) has found that five or even six different photosensitive processes in the eye may be necessary for interpreting the effects of chromatic adaptation. Yet the five or six kinds of receptor may merge their responses into three channels in the nervous system.

Ekman (54) has undertaken to fit normal probability curves to transforms from the statistical factor loadings derived from a matrix of subjective

similarities among spectral colors. Five such curves are used. Von Schelling (163) has fitted normal probability curves to Boynton's (21) recent data on chromatic sensitivity functions. Boynton had concluded in favor of a minimum of four components, whereas the von Schelling treatment is for only three.

A novel theory of color vision is put forth in an article by Shaw (138). After asserting that psychologists are not very knowledgeable with respect to optics, and that no one scientist can develop a sensible theory of vision, he launches into a discussion of anomalous rotary dispersion. This is an optical effect characteristic of certain dyes. One of these dyes is assumed to be the basic photochemical substance in all the cone receptors. Having a maximum absorption in the green, its refractive index varies widely for wavelengths near this region. Rotary polarization occurs on either side of the maximum, the extent of the polarization falling off toward each end of the visible spectrum. Complementary colors are those whose rotations, right and left respectively, are balanced against one another. Color blindness results from minor variations in the structure of the dye molecule. Afterimages result from a reverse or return activity within the molecule after its arrangement has been temporarily altered by the action of strong light. Shaw does not attempt to discuss the role of single cone receptors in a system whose basic substance is common to all the cones. One may wonder, for example, how one cone can signal at one time the color of a stimulating light and a little later the color of its afterimage. Are there multiple nerve fibers for each cone, or is the signal from each modulated in some way to transmit different responses along a single path?

Willmer (174) puts forward the hypothesis that red and green vision are mediated by specialized cone receptors but that the blue mechanism has to do with the rods. Specifically scotopic vision is primarily rod vision, but photopic vision involves both rods and cones. Cone activity somewhere, perhaps through inhibitory effects on the ganglion cell bodies, serves to inhibit rod activity. Thus vision for blue is provided by high-level responses of the rod receptors to blue light, i.e., to light that does not stimulate the cones sufficiently to cause them to suppress the rod responses. Willmer regards the blue mechanism as a "primitive" one that is absent in the human central fovea but present in most forms of color blindness.

Burnham, Evans & Newhall (34) have described a procedure for computing the color appearance of everyday objects. This takes into account the illuminant to which the observer has become adapted. Newhall, Burnham & Clark (111) have compared the results of successive and simultaneous matching of colors. The "memory method" of successive matching is in wide practical use and is found to be more rapid than the simultaneous matching. Standard colorimetry usually involves simultaneous matching, and this is found to yield more consistent results. Chapanis & Halsey (36) have recomputed some earlier data on color coding, using a multivariate logarithmic

contingency analysis. Brown (32) reports on the distribution of color-matching errors for 22 colors distributed throughout the chromaticity diagram, using an improved binocular colorimetric device.

Dimmick (47) gives data on the current edition of the ISCC Color Aptitude Test. Wright (178) remarks on the discrepancies that still exist among readings given by various physical spectrophotometric instruments. Pickford (120) has developed a reasonably simple anomaloscope with filters for testing red-green and yellow-blue color vision. He explains the instrument and its use, pointing out the diagnostic value of such a test as compared with other tests of color vision. Walls & Heath (168) have used Munsell papers and a color wheel to mix blue and green and so to find neutral points for color blind subjects. This procedure minimizes the effects of ocular pigmentation and makes it possible to diagnose protanopia and deuteranopia differentially from the neutral-point measurement alone. Mann and Turner (97) have found a very low (1.9 per cent) incidence of color defect in male Australasian aboriginals as compared with 3.2 per cent in half-castes and 7.3 per cent in white men.

Stultz and Koch (144) have found that as applied to color materials, the methods of specifying graininess in black and white materials are inadequate. Color differences among the grain patterns must be included in the analysis.

THE ELECTRORETINOGRAM (ERG)

The current year has seen a burst of activity in the recording of the ERG, some 58 papers having been seen by this reviewer. A few of the papers are mainly concerned with pathological conditions of the retina, and are not included here. Some of the remainder are in a collection available as a separate number, entitled "Elektroretinographie" (135), of the *Bibliotheca Ophthalmologica*. This is a group of 19 papers by European investigators who participated in a symposium in Hamburg in 1956.

Armington & Biersdorf (8) flooded the human eye with colored light for prolonged intervals and then superimposed a flickering stimulus. The resulting ERG responses showed identifiable photopic and scotopic components, but specific chromatic effects were quite small. In a later study (16) with single test flashes these authors succeeded in achieving specific chromatic adaptation effects defining two response systems with maxima near 500 and 620 m μ . This significant result indicates the possible action of two photopigments, rhodopsin and cyanopsin.

Flicker ERG experiments have also been performed by Goodman & Iser (63), who report only low-frequency rod responses in a totally color-blind subject, and by Wadensten (165) who notes that normal cone responses of high frequency may be present in cases of rod deficiency where the responses to single flashes are much reduced. Henkes, van der Tweel & van der Gon (74) have applied the frequency analyser technique to the measurement

of ERG flicker responses in normal human subjects, patients, and animals. They show the advantages of the method in picking up flicker responses that are otherwise too small to be measurable.

Johnson & Bartlett (86) in a study of exposure duration, have shown that height of the *b*-wave of the ERG increases with duration up to about 0.1 sec. in the dark-adapted eye, and 0.03 sec. in a partially light-adapted one. These times may be regarded as fairly well defined critical durations, below which height of ERG is roughly proportional to log energy of flash. The results are similar to those found in psychophysical studies. Wirth (175) also reports an exposure duration study for the eye in dark and light adaptation, with results in harmony with those just cited.

Alpern & Faris (6) have measured human *a*- and *b*-waves in response to flashes of varied duration. They conclude that the *a*-wave magnitudes are in harmony with the hypothesis of a simple photochemical response while the *b*-wave magnitudes are not. This may indicate the origin of the *a*-wave in the outer regions of the photoreceptors and the *b*-wave in the inner regions, as Noell has suggested.

Bornschein, Goodman, & Gunkel (18) report response time and amplitude data on the *b*-wave in the dark-adapted eye that show clearly its scotopic nature even in response to red light. Bornschein & Gunkel (19) have also varied the rate of onset of the light stimulus for the ERG and have found that a slow rate of onset prolongs the *b*-wave and eliminates the *a*. Ronchi & Grazi (128) have also varied rate of onset and recorded the results for light- and dark-adapted eyes. Unfortunately the last two studies have used capacitance-coupled amplifiers to record their results. While the time constants were long, it would seem mandatory to use direct coupling for studies in which such long durations are being observed. Best & Bohnen (15) have used DC recording in a thorough study of human dark adaptation as shown by height of *b*-wave. Clear cone and rod portions of the process are revealed, the cone-to-rod transition occurring in from 5 to 10 minutes of adaptation, depending on test flash intensity. Using a 25 μ v. ERG as criterion, the resulting sensitivity curve closely resembles one obtained by a psychophysical threshold technique under identical conditions.

Crampton (40) finds new evidence that the ERG is importantly affected by stray light, and also confirms the finding that a feebly illuminated retina may exhibit larger ERG's in response to a given test flash than a retina that has been in complete darkness until the flash is given.

ten Doesschate & ten Doesschate (157) measured the amplitude of the human resting potential by oculo-graphic recording. They confirmed the slight drop during early dark adaptation and rise during light adaptation that others have reported, but noted a secondary rise during late dark adaptation. Henkes (73) discusses the influence of retinal metabolic condition on the form and magnitude of the ERG.

Motokawa, Nakagawa & Kohata (105) have measured the human ERG

in one eye under two conditions. In the monocular condition the same eye was stimulated with light. In the binocular condition both eyes were stimulated. The resulting responses were slightly different, especially in the later portions coming after the *b*-wave. Motokawa argues that these differences are the result of efferent neural activity coming to the recorded eye from the other one when both are stimulated. Unfortunately, no records are given of responses from monocular stimulation of the other eye. In this reviewer's experience these late portions of the record are highly variable and may be the result of winks or other reflex muscular activity whose magnitude might be different with binocular and monocular conditions of fixation and stimulation.

While most of the research just reviewed on the ERG has been concerned with human subjects, this reviewer agrees with Granit that fundamental work is usually best accomplished with animals. Psychologists of course are fascinated by the possibility of comparing electrically recorded responses with psychophysical data from the same human subject. Perhaps one of our main tasks in the future should be that of pursuing behavioral studies of visual discrimination in animals whose basic visual functions and structures can also be studied in detail by electrical means.

Great progress is now being made in microelectrode recording from within the eye, rather than by the use of external electrodes. Brindley (24, 27) using the excised eye of the frog, has made a study of some aspects of the electroanatomy of the eyeball. He has also found the effect of stimulus area on the ERG (25) to be approximately an additive one with little evidence for effects of neural summation. Central and peripheral areas yield similar responses, and Weber's law holds as shown by a constant ratio of stimulus to background intensities of light over a moderate range.

In another carefully conducted series of experiments Brindley (26) has followed up the earlier work of Tomita and has penetrated the frog retina to various depths in an effort to localize the origin of the ERG. Contrary to Tomita, he now believes that the ERG consists of potentials generated in the rods and cones. Other waves are found in the nuclear layers of the retina, as Tomita has reported, but these he believes to be localized potentials originating perhaps in the horizontal interconnecting cells of the retina and not contributing to the ERG as it is usually recorded.

With Torihama, Tomita (161) has extended his earlier research on the frog retina and has reaffirmed his conclusion that the ERG is generated primarily at the depth of the bipolars rather than the receptor cells. In this study and in one on the Limulus photoreceptor (159) Tomita has made use of his ingenious "pencil-electrode" for simultaneous internal and external cellular recording.

Ratliff (124) reports a preliminary experiment on the ERG of the sea scallop. The wave form is relatively simple, with no off-component, despite Hartline's earlier finding that optic nerve fibers from the distal retina re-

spond only to the cessation of illumination. Dark adaptation takes place within about 40 min.

Armington & Thiede (9) report an ERG study of the chicken that gives spectral sensitivity data in conformity with a 1921 behavioral study by Honigsmann. There is a Purkinje shift, from a spectral curve partly agreeing with rhodopsin for the dark-adapted eye to one near the iodopsin curve in light adaptation. Keidel (88), using a glare source on the frog eye, confirms the stray light effect and the similarity of ERG from various regions. He also reports (89) duration effects in responses to a flickering light. Arden & Greaves (7) report that, contrary to an early study by Granit, the *a*- and *c*-waves are the first to be affected by retinal ischaemia in the rabbit, the *b*-wave being more resistant.

Zetterstrom (181) reports that kittens attain a normal ERG by about the eighth week after birth. Interestingly enough, however, animals reared in the dark did not begin to develop an ERG until the third week, while animals in the light began at one week. It is suggested that phosphorylation is required for the development of the ERG and that early development is promoted by the action of light.

NERVE IMPULSES

The use of fluid-filled micropipette electrodes permits recording from single cell bodies, at least for a matter of minutes. The most astonishing achievements of this kind are reported in a series of papers (148 to 153) by Svaetichin. This author, whose earlier work has already stirred up considerable controversy with Granit, Tomita and others, now describes experiments which, if confirmed, would go a long way toward solving the age-old riddle of color vision. Briefly, Svaetichin claims to have inserted single ultra-micropipette electrode tips into single cones of fish eyes. The resulting responses are recorded to light from some 24 regions of the spectrum as provided by selective interference-type filters.

The pattern of response magnitudes is found to be one of three types. Type L, with maximum response at about 574 m μ , appears to be a luminosity response mechanism responding broadly to light throughout the spectrum with increased intracellular negativity. Type R-G shows intracellular positive responses for wavelengths shorter than about 590 m μ , and negative responses for the longer wavelengths. The positive maximum at 506 m μ and negative at 639 m μ have suggested the reference to a red-green receptor. The Y-B type, exhibiting a polarity change at about 560 m μ , is negative for short wavelengths (maximum at 460 m μ) and positive for long (maximum at 610 m μ). Here, then, is a latter-day physiological vindication of Ewald Hering, whose picture appears as a frontispiece for the Svaetichin papers.

The implausibility of electrophysiological responses of dual polarity has always been a major objection to the Hering theory. Svaetichin appeals to the existence, in these fish, of double cone receptors. These structures, as-

sumed to be the ones showing R-G or Y-B responses, are seen under a microscope to be in close contact for most of the length of the ellipsoid. The assumption, then, is that the two twins produce potentials of opposite polarity. The membrane of one twin, maintaining a sort of synaptic relationship with the membrane of the other, can impress its potential across the synapse. Thus the green or yellow twin acts with depolarizing action, the red or blue with hyperpolarizing; and an electrode inside either twin would be similarly affected. Presumably the Type L cone is a single type.

Svaetichin admits that twin cones have not been found in the human retina. He believes, however, that the twin-color-receptor scheme holds generally throughout the animal kingdom and that there may be the necessary synapses among single human cones. As additional evidence he cites spectral polarity effects reported for phosphene experiments with electrical stimulation of the human eye and the psychophysical data that have always been used in support of the Hering theory.

Svaetichin's work will certainly not go unchallenged. The stimulating and recording equipment that he has assembled in his new Venezuelan laboratory, admirable though it is, has its counterpart in several other places where such work is being pursued, and we may expect progress whatever the verdict on the work that he has just reported.

Tomita (160) already reports an attempt to record intracellular potentials within the receptor layer of a fish retina. While he did not vary the wavelength of the stimulating light, he did apply white light of moderate intensity. Ultramicropipette electrodes were made to penetrate the retina from the receptor side or from the vitreous side, and responses similar to those reported by Svaetichin were found. Always, however, these responses came from a depth of 160 to 170 μ from the tips of the receptors and appeared histologically to be proximal to the receptors. Tomita concludes that these are the intraretinal action potentials that he has also found in the frog. Furthermore, he finds that these potentials can also be recorded by the use of coarser electrodes, indicating that they cannot be coming from within cone receptors as claimed by Svaetichin.

Granit (65) has reviewed the recent progress of "retinology," emphasizing the tie-in between electrophysiological studies of receptor sensitivity and biochemical analysis of the photopigments. Also by Granit (66) is a summary of the facts regarding centrifugal impulses reaching the retinal ganglion cells and causing a potentiation of the responses to light. Dodt (50) finds the conduction velocities for these centrifugal impulses to be from 1.7 to 2.8 meters per second, proceeding along unmyelinated nerve fibers from the blind spot to ganglion cells at various points on the retina. In another paper Dodt (51) establishes more definitely the true centrifugal nature of these impulses in the rabbit, showing that they have traversed one or more synapses.

Dodt (52) gives an account of an "inhibition due to overstimulation" in the completely dark-adapted eye of the rabbit. Dodt & Elenius (53) find

no evidence of a Purkinje shift in single-ganglion cell recording from the rabbit retina. Instead there is a slight shift toward greater blue sensitivity with higher light adaptation. It seems possible that this may again represent the inhibitory effect of blue light in the dark adapted eye, though Weale has indeed found similar wavelengths of maximum light absorption in the guinea pig photopigments.

Grusser & Creutzfeldt (67) have reported that in the cat there is a maximum cortical response to flickering light at about 9.2 c.p.s. This is true for single "on", "off", and "on-off" fibers, as measured by frequency of nerve impulses. This is regarded as objective evidence for the fact that flickering light in this frequency range may seem more intense than light at other frequencies or the equivalent steady light (Brücke-Bartley effect).

That a mechanism for the enhancement of contrast exists in an invertebrate eye is shown in a paper by Hartline, Wagner & Ratliff (71). In the compound eye of *Limulus*, responses from one ommatidium may be reduced or abolished by illumination of adjacent ommatidia. The site of the interaction is within the inhibited ommatidium itself, presumably by virtue of signals brought to it along interconnecting fibers that come from adjacent ommatidia. These signals have not yet been recorded directly and their nature is not yet understood.

Further experiments by Hartline & Ratliff (70) have revealed that mutual interaction may occur between neighboring ommatidia, so that any one responds more vigorously when stimulated alone than when stimulated simultaneously with others. It is also shown that the power of a given unit to inhibit another unit is determined by the degree of its activity. As a consequence of this, three units may appear to interact in a more complex way. For example unit A may so inhibit B that B is less capable of inhibiting C. Thus if C and B are illuminated, the rate of response of C may be increased by illuminating A. The apparent facilitation of C by A is therefore a case of disinhibition. A set of simultaneous equations may be used to describe the activities of individual units that are interdependent in this way.

The purely inhibitory character of interaction in *Limulus* is further attested in an experiment of MacNichol & Benolken (95). They find that ethyl alcohol may be used to reduce or eliminate the inhibition without affecting the regular response to light. Thus the disinhibition produced by alcohol may result in an increased responsiveness to light.

Burt & Catton (35) have made a study of responses in the optic lobes of insects. They have recorded both slow potentials and spikes in response to light, and have also found rhythmical sinusoidal waves of a spontaneous nature.

A general review of vision in the compound eye is given by Wulff (179). Topics included are color vision, pattern vision, pigment migration, and specific responses to polarized light.

A readable and authentic article on electrical events in vision is one by

Milne & Milne (100). Also by the same authors is a chapter (101) on invertebrate photoreceptors which has appeared after a five-year delay in publication. In the same volume and equally out of date is a chapter by Riggs (126) on the subject of electrical phenomena in vision.

EYE MOVEMENTS

During monocular fixation on a single object the eye exhibits wandering movements ("drift") and rapid displacements (saccades). Cornsweet (39) has measured these movements by the use of a plane mirror mounted on a contact lens. In some of his experiments normal viewing was used, but in others the retinal image was rendered stationary on the retina by the "stopped image" technique for counteracting the eye movements. In these stopped image experiments, the extent to which the fixation object disappeared was regulated by flickering it at various rates. It was found that (a) the frequency, extent and direction of saccades was related to the position on the retina of the image of the fixation point, (b) drifts are not dependent on image position, (c) saccades are not triggered by disappearance of the fixation point, and (d) proprioceptive feedback has no obvious role in maintaining fixation on a stationary point.

Ditchburn (48) has described further experiments in which a stationary retinal image was obtained by the plane-mirror contact-lens technique. He has also used various rates of flicker to study restoration of vision after it has disappeared due to lack of image movement. Flicker at about 20 c.p.s. is most effective. In other experiments, the precision with which perfect stabilization is achieved is reported to affect the results, to the extent that an image movement of 2 parts in 10,000 of normal movement is significant. A very small bright image, motionless on the retina, might be expected to appear in different colors as it stimulates individual cones or small numbers of cones, yet this was not found to be true. Ditchburn and his co-workers (49) have also described a technique for stabilizing on the retina an image of the iris, without the necessity for an individually fitted contact lens.

Alpern & Ellen (5) have occluded one eye and measured its movements of convergence when the other eye accommodates to different target distances. The occluded eye moves smoothly and continuously inward, and the stimulated eye does not take part in the convergence movement. Measurements are made of the latency, duration, amplitude, velocity, and acceleration of the movement. Alpern (4) has also induced a convergence movement by placing a prism before one eye. The recorded magnitude of eye movement was always smaller than the stimulus, thus accounting for preceptual distortions that occur during prism vergence tests. Westheimer & Mitchell (173) have induced movements of convergence by varying the separation between stereoscopic images for the right and left eyes. The eyes exhibit two kinds of response movement, a conjugate saccadic one and a somewhat slower movement of convergence.

Tani *et al.* (154) report evidence that it is not possible to measure precisely the movements of the eyes by photographing corneal reflections or spots on the eyes themselves. One reason is that the eyes appear not to rotate around fixed points. Instead they exhibit translatory movements in addition to rotary ones in response to the extraocular muscles. Thus these authors are in agreement with Ratliff, Riggs, and Ditchburn who have abandoned these methods in favor of the one in which collimated light and a plane mirror can be used to minimize the effects of the translatory movements.

Christoferson & Ogle (37) have studied the extent of accommodative convergence with varying degrees of cycloplegia. Homatropine does not change the fusional amplitude but increases the ratio of the accommodative convergence to the accommodative stimulus change. It is thought that the drug may depress inhibitory mechanisms that normally modify the accommodative convergence responses. ten Doesschate (156) has described pendular nystagmus in one eye under conditions suggesting that both eyes are involved but that the responses of the other eye are too small to be recorded.

Pheiffer, Eure & Hamilton (119) have obtained evidence that the reversals of reversible figures occur prior to eye movements and not vice versa. Unfortunately their argument is somewhat weakened by the fact that their timing included the reaction times of both subject and experimenter. Singer (139) has described an electro-mechanical eye-movement simulating device based on visual feedback, and Schade (136) has also devised an electrical model of the eye.

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HEARING^{1,2}

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INTRODUCTION

The past year was an especially interesting period in audition. Several very young laboratories in this country, at Indiana, Michigan, Cleveland, and several other places, showed a very solid output, with no diminution from the more established laboratories. Evidently the new groups in this country are not forming at the expense of established staffs, but represent the addition of significantly new thinkers in the field.

The situation is even brighter for certain other countries. Quite a number of absolutely first-rate papers appeared from Japan, and in the *technische Hochschulen* and otological clinics of West Germany a great deal of high-quality psycho-acoustics is going forward.

But while audition researchers can take comfort in an expanding economy, psychology as a science is very rapidly losing its former relatively-high status in audition. A wealth of data is now appearing from engineers, audiologists, otologists, biochemists, and physiologists, who have of course their own communications outlets. Even explicitly psychologically-oriented studies are no longer usually presented to psychological audiences either at meetings or in the psychological journals. For example, at the 1956 Annual Meeting of the American Psychological Association, in contrast to the three full sessions on vision, there was no session on audition. Four very minor auditory studies were scattered in sessions which would not be expected to draw a specifically informed audience to these papers. For another example, this review contains only three references to the American psychological journals. This lamentable state of affairs does not mean that psychologists are not working as hard as before; it quite obviously means that some policy or policies of the American Psychological Association, relative to its annual meetings and its journals, is about to dry up—nay, has already dried up—the flow of psychologically-oriented auditory papers into traditional psychological channels.

It was a banner year for reviews and original papers. General reviews included the fine one by Lawrence (98) covering two years, and including others by Békésy (15) largely on cochlear mechanics, Newman (125) for a physics audience, Ranke (139) for otologists, Tasaki (158) largely on the electricity of the cochlea, and Davis (38) referred to below. Markle & Fowler

¹ The survey of the literature pertaining to this review was completed in April, 1957.

² In this chapter the following abbreviation is used: DL (difference limen).

reviewed the related field of audiology (111). Well over a thousand articles appeared which are of some interest to the specialist. It is necessary to sift several times, with prearranged biases in mind, to obtain an appropriate yield. Many worthwhile studies must in any sifting be eliminated. This often hurts the reviewer as much as it must the author.

There is a current notion that "auditory theory" concerns patterns of cochlear mechanics plus the tonotopography of the auditory cortex. To the present reviewer this is a too foreshortened perspective. All aspects of audition can be grist. The psychology of audition is in one important sense not reducible to neurophysiology or electrochemistry. However, it is of course impossible to understand the operation of a sensory system without a clear notion of the functional anatomy and broad physiology of the peripheral organ and of the sequence and organization of neural events. This review will mix data from psychology proper and from the supporting sciences in proportion as seems necessary for mutual elucidation.

FUNCTIONS OF THE EXTERNAL AND MIDDLE EAR

A study by Fiori-Ratti *et al.* (50) on the halved heads of cadavers shows that localization is based partly upon the various interference effects produced by the pinna. Konig & Sussman (93) have clearly shown that head movements are essential to the best localization in the median sagittal plane. Even though subjects were certain their heads were immobile, there were two types of movement, one attaining 3° at irregular intervals, and another reaching 5° synchronous with the pulse. When head movements were eliminated, directionality was reduced. The authors implicate the pinna. Tatsumi (159) has also added data on this point.

Kraus (94) gives five common observations on localization which he contends cannot be accounted for by a time theory. Other worthwhile papers on the topic are those of Kikuchi (86) on methodology, Schodder (143), and Duyff (41).

The Wever laboratory has studied (138) the effect of pressure in the external auditory meatus. Under increasing pressure the eardrum becomes more and more like a stiffness-controlled membrane. As pressure increases, the transition frequency (at which the eardrum breaks down as a simple membrane) rises from about 1800 c.p.s. to several thousand c.p.s.

Zwislocki (179) outlines three methods for measuring impedance at the eardrum. In the handiest method a sound-transmitting-and-measuring system is inserted into the meatus and then into a small cavity of known impedance. Wagemann (169) uses such data in the clinic. This country has been rather behindhand in exploring the clinical possibilities here.

Gisselsson *et al.* (58) and Beickert *et al.* (14) used a neuromuscular blocking agent, succinylcholine, on the middle ear muscles. It happens that in the cat this drug causes contraction of both middle ear muscles. At maximum, the drug affected all tones (250 to 9000 c.p.s.) about equally (earlier workers had supposed no special effect above 1000 c.p.s.). When both tendons were

cut, the depressant effect of succinylcholine entirely disappeared. Human and guinea-pig intratympanic muscles do not react like the cat's. Incidentally, these muscles have many characteristics not like those of other muscles in the body. Eliasson & Gisselsson (43) recorded latencies and amplitudes of diphasic potentials from both muscles. The stapedius reflex is more sensitive (about 20 db), responsive to a broader spectrum (about two octaves), shorter (less rostral) and therefore about 1 msec. faster. Repeated sounds have a tendency to decrease latency by about 1 msec., perhaps due to facilitation in the internuncial neurones.

The middle ear is very thinly separated from the inner ear. The round window is practically an open route (110). There may be direct nervous connections (142). Everberg (45) gives a review of the consequences of otitis media for cochlea and labyrinth. This route is indeed not infrequently the cause of infant death (36). Loeschcke (106) discusses the absorption of gases in the middle ear, with diffusion into the fluids of the cochlea. Wing (173) has shown that flushing the bulla with nitrogen will sharply reduce the postmortem microphonic in the cat.

LABYRINTH CHEMISTRY

The chemical compositions of the perilymph and endolymph are receiving continued attention. Casorati & Posarelli (26) have extended earlier observations with the electron microscope. Churchill *et al.* (34) bring us up to date on acetylcholine in the cochlea. It is evident that acetylcholinesterase is abundant in the cat's organ of Corti, more especially in the region of the nerve chalice in which the hair cell rests [see Portmann (135)], and more abundantly for the inner than the outer rows. However, from previous data on acetylcholinesterase in afferent and efferent nerves, the authors suggest that the abundant amounts are related more to the efferent nerves of the olivo-cochlear tract. Galambos (55) finds that electrical stimulation of the roof of the medulla will suppress N_1 , the first neural event in the cochlea, but he leaves open the question of how this is accomplished. Churchill *et al.* suggest there may well be a humoral inhibition of the nerve-hair cell junction. This would neatly explain Galambos' finding.

Marco *et al.* (110) applied hyaluronidase to the round window of the guinea pig but found no change in the audiogram; nor did histology show any tissue abnormalities. It is, therefore, most probable that the endolymph of the mammal is much less viscous than that of the elasmobranch.

Fernandez has thoroughly documented (48) the effect of hypoxia and anoxia on cochlear microphonics. The broad features are now well known; further work can only fill in relatively minor details. The real question now is, what is being oxidized? Fernandez & Brenman (49) consider blood sugar. Neither cortical nor microphonic responses were affected by quite drastic reduction of blood sugar, until blood pressure fell below shock level. The microphonic output of cats reported by Wing (174) fell off at blood sugar levels considerably above those reported by Fernandez & Brenman. A

positive result takes precedence over a negative, and the fact that Fernandez' cats could maintain microphonics at low levels of blood sugar would lead one at first to seek for another cause than hypoglycemia for the decline of Wing's cats. It does not seem likely that there exist large reserves of glycogen within the cochlea. Other metabolic systems could, of course, come into play. The question of what is oxidized in microphonic production thus remains open.

By paper chromatography on the perilymph of the calf, guinea pig, and human, Chevance, Galli & Jeanmaire (30) established a group of low molecular weight acids all of importance in various phases of glucose metabolism. Flooding the guinea pig with intravenous pure amino acid for three days did not disturb the balance among the amino acids normally present. It is concluded that the balance is constant, probably homeostatically controlled by enzymes. It would be interesting to repeat this experiment with animals in quiet and in noise. These same authors (29) have studied the perilymph proteins with an electrophoresis technique. Casorati & Crifò (25) have also used electrophoresis and refractometry to study perilymph proteins. In a 100 ml. sample they found 1.37 gm. of globulins and 0.28 gm. of albumins. Antonini, Casorati & Crifò (4) establish that the perilymph is more allied to blood than to cerebrospinal fluid; this rather suggests that perilymph is not derived from cerebrospinal fluid as commonly supposed, but from blood through some selective filtering process. Citron *et al.* (35) have confirmed earlier work to the effect that endolymph is relatively high in potassium, low in sodium.

These studies have the most direct consequences for the metabolism and electrochemistry of audition. What we are accumulating is a picture of complex barrier structures (or processes, the blood-endolymph barrier for example not being a true structure) able to exclude both metallic ions and also certain free amino acids. The best guess seems to place the perilymph-endolymph barriers on the one side at Reissner's membrane of a certainty, and on the other side probably at the lamina perforata. Note that this would leave the hair cells bathed in perilymph except at their tips.

NEUROPHYSIOLOGY AND PHARMACOLOGY IN THE HUMAN EAR

Knisely & Lockard (89) discuss how circulatory and vascular pathology can affect the inner ear and auditory nervous system. The autonomic nervous system is extremely important. Beickert (12) relates cervical sympathetic distribution and clinical hypacusis. A review of the role of the autonomic nervous system and cochlear blood and circulation is given by Shuster (146). Rawlins reviews the role of the corticosteroids (140), cortisone-like hormones, in hearing. The corticoids aid in maintaining healthy capillary endothelial cells. A breakdown in the capillary sheath would have profound influence on the cochlea. Hofer (69) reports improved hearing in elderly people from sex hormone injection, presumably due to improvement in general circulation.

Vitamins A and E have been reported beneficial to hearing (151), and success has been reported with heparin (e.g., 96). Quite a few workers suggest therapy consisting of salt-free diets and the like (e.g., 131, 132) in an effort to re-establish normal hydrodynamic conditions. Streptomycin continues to be cautioned against (for a recent review with experimental data see 165). The role of several diseases which result in sludging of blood in peripheral vessels has again been strongly emphasized by Fowler (53). Among diseases with accompanying disturbances of nutrition and metabolism and which may cause hearing defects, recent reports have discussed diabetes (2), the Rh factor (60), goiter (120), mumps (167), and hypothyroxinemia (8). Reviews have appeared discussing hearing defects with respect to a wide variety of virus diseases (74) and neurogenous disorders (121).

These and scores of similar experimental as well as clinical articles emphasize that the auditory mechanism can no longer be regarded as an almost inaccessible *terra incognita* beyond the reach of any technique except the otoscope and the mastoid chisel. (We leave altogether out of this chapter the really tremendous strides in middle ear surgery in very recent years.) We speak here of a growing realization that cochlear transduction is critically dependent upon general, cerebral, and peripheral circulation, upon correct intricate hormone balances, osmotic conditions, vitamins, and respired gases, and can be upset by imbalances in any of these areas, or even by everyday conditions such as allergies, mild as well as severe viral attacks, and sub-clinical infections of the middle ear.

This view of cochlear physiology is rather dramatically shown by studies on sudden deafness. A short while ago there was little hope, and treatment was perforce perfunctory. Today, causes are actively being sought from many points of view and treatment is beginning to be placed on rational grounds. Several of the papers in this area are comprehensive enough to list here (11, 62, 105, 149, 152).

NEUROPHYSIOLOGY AND BIOCHEMISTRY OF THE ANIMAL AUDITORY SYSTEM

Many workers have sought to explain some of the human material just outlined, by appeals to the animal. The critical importance of O_2 for both the AC and DC components of cochlear electricity has been again satisfactorily assessed by Toyoshima (164) with differential electrodes in the guinea pig. Kawata *et al.* (84) have shown how stimulation and blocking of the superior cervical ganglion can diminish or augment the AC microphonic (presumably by a vasomotor effect on the cochlear circulation). Beickert & Beck (13) prepared guinea pigs with a sympathicolytic drug (Regitin) or with a sympathicomimetic drug (Ephedrin), then subjected them to 3000 c.p.s. at 120 db for from 30 min. to 1 hr. They found that Ephedrin at first increased sensitivity, then later the initial increase turned to a decrease. Regitin produced only an initial decrease in sensitivity. Beck (10) exposed guinea pigs to sounds at 30 db for 36 hr., other pigs up to 120 db for 1 hr., then looked at

the nuclei of the outer hair cells with especially thin sections of 2μ . Upon reconstructing the loss in the cochlea, he found the usual widespread effect with a pure tone.

The close dependence of microphonics on metabolic processes underlies the quick decline upon local cooling of the cochlea, and the quick recuperation upon warming (28).

Kimura & Perlman (87) have simulated certain human disorders by completely obstructing the inferior cochlear vein of the guinea pig. Within an hour or less, dilatation of capillaries and edema of epithelium in the stria vascularis could be seen histologically. Outer hair cells, inner hair cells, and spiral ganglion cells were lost in that order. The major membranes were largely unaffected. Takahashi (155) took the approach of measuring perilymphatic pressure, working down to a low of 1.082 mm. H_2O with consequent effects on microphonics. Hibino (68) looked at the stria vascularis with respect to gas exchange.

Several methodological papers have appeared. Beck (9) has copious notes on histological preparations for viewing the nuclei of the spiral ganglion. Chocholle & Legoux (32) show how electrode placement can allow action potentials to intrude unwanted upon a measure of distortion in cochlear microphonics. Legoux *et al.* (101) show that even with a steady state stimulus, the harmonic content of the microphonics will vary with time. Pestalozza & Davis (130) have used very high audio frequencies in order to utilize the round window as an electrode placement for simultaneous recording of microphonic, summing potential, and action potential. Matsuoka (112) and Matsuoka *et al.* (113) contribute what will be the classic papers on the electric impedance of the cochlea. Since the impedance of living tissue varies with frequency, obviously a correction factor must be applied to data relating frequency to microphonic output. Such data are provided. Pure tones were led at constant intensity to the ear canal, and recording was by differential electrodes from the second turn. By adjusting the magnitude of a grid leak in a preamplifier, the impedance of the whole system could be estimated if their ingenious electrical model is correct, and by further measuring the phase lag between microphonic output with respect to acoustic input, the authors were able to estimate the impedance constants up to, and also within, the cochlea. It develops that the reactive and resistive components of cochlear impedance are linear functions of log frequency. Knowing the impedance, one can calculate the generator potential in millivolts. Its magnitude is fairly constant at .45 mv. up to 1000 c.p.s. but then falls off rapidly to about .15 mv. at 10,000 c.p.s. The electrical energy consumed, in watts, follows a similar course.

It should be said that these data must eventually receive another correction, from Wever's body of papers, on the relation between constant sound energy at the ear canal (where Matsuoka measured it) versus the energy delivered to the perilymph (where the cochlea can use it) as a function of frequency.

Woellner & Schuknecht (175) cut the auditory nerve in the cat deep within the internal auditory meatus, and tested with behavior audiograms. A low frequency loss was produced in one animal, with a concomitant widespread loss of apical spiral ganglion cells, but with normal audiogram at high frequencies, in spite of a loss of 60 to 75 per cent of spiral ganglion cells in the basal turn. We have here two pleasant surprises. Four other cats handled similarly seem to delineate the principles that, first, partial section of the nerve can differentially affect low frequencies, as makes very good sense, and second, that something like 75 per cent of the spiral ganglion cells must be lost before a certain frequency region shows audiometric loss. On two cases of VIII nerve tumor in the human, speech reception was strikingly worse than the pure tone audiogram alone would predict. Evidently a quite large safety factor is built into the neurology of the absolute intensive threshold.

Vernier & Galambos (168) explored the single nerve fibers of the medial geniculate in the cat with a view to studying changes in response to clicks under different drugs. Galambos (54) has summarized a number of his more recent experiments, particularly on the olivo-cochlear pathway, for a medical audience. Papers have appeared from Neff's laboratory on the role of the auditory cortex in pitch discrimination (23) and localization (124) and on smooth slow waves in the cat's cortex in response to continuous acoustic stimuli (91). This is presumably the same phenomenon Kohler & Wegener (92) pick up from the top of the head in the human with a direct-coupled amplifier. It is Kohler's view that cortical projection areas maintain such slow currents by synaptic electromotive forces. [For an extension of the usual notion of what the auditory projection areas are, see Kiang (85).]

The review by Davis (38) cannot be passed over. It is a fascinating summary of and full commentary on the biophysics and physiology of the inner ear. He presents his own interpretation of the electro-anatomical facts concerning the origin, interrelations, and consequences for hearing of the seven recognizable types of electrical current found within the cochlea, (a) the usual microphonic, (b) the post-mortem microphonic, (c) the usual intracellular negativity of probably all cells within the cochlea with respect to surrounding fluids, (d) a resting DC potential difference between the endolymph and tectorial membrane versus all surrounding tissues and fluids, (e-f) both positive and negative changes in (d) as a result of acoustic stimulation; these changes Davis has named the "summing potentials," and finally (g) the action potential spike of the first neurone.

Particularly new and interesting are Davis' views on the summing potentials. These are certainly tied to mechanical movement of the cochlear partition, as Békésy showed, but they can be dissociated from the resting DC potential and from the cochlear microphonics because a negative summing potential can exist during hypoxia when the other two electrical events have been lost. Furthermore, in contradistinction to the cochlear microphonic, they do not alternate in phase with an acoustic stimulus but are unidirectional. They arise in the hair cells as a result of an unsymmetrical mechanical

process in the shearing action between the tectorial membrane and the reticular lamina. In the case of the microphonic the motion of the cochlear partition is transverse and symmetrical. Now it is suggested that a wave travelling longitudinally up the cochlea might, if strong enough, easily produce an additional longitudinal motion of the tectorial membrane. Such a longitudinal motion would add a new dimension to the stresses upon the tectorial membrane, resulting in nonlinearity and overloading of the microphonics, and a new potential, the negative summing potential, would enter the picture.

According to Davis' theory, the negative summing potential could serve to stimulate auditory nerve fibers (or drastically lower their threshold) at acoustic energies where the symmetrical vibrations of the cochlear partition were beginning to break down into asymmetry. At about this level the negative summing potential appears clearly and grows linearly up to at least 10 mv. This furnishes the possibility that it, so to say, takes over (or contributes in much greater proportion) to stimulate the nerve fibers up to acoustic energies injurious to the ear. In this way the dynamic range of the ear is enormously increased.

OBJECTIVE AUDIOMETRY

Gersuni (57) summarizes much of the Russian work in this field. Foetal movements were recorded by Fleischer (52) from 79 women in the last stages of pregnancy, with tones 500 to 1000 c.p.s. at 115 db, a foot or two distant. Wedenberg used the eyeblink reflex to sound, and the sound intensity required to wake from sleep, on infants up to three months old (172). Enough consistency was found among 20 normal-hearing neonates to make it possible to assess correctly normal hearing, a flat-frequency hearing loss, or an abrupt high-tone loss, all data being highly important in diagnosis.

Derbyshire *et al.* (40) review all previous work on audiometry by electroencephalogram, and add further data on sedated children. They distinguish four components of the response, an on-effect, a continued response, an off-effect, and a delayed arousal effect. The authors propose a nonspecific subcortical arousal center which has split off from the primary sensory projection pathways to the cortex.

Autonomic responses such as plethysmography (160), heart rate (176), and PGR (21, 108), have not been neglected, though Goldstein (59) points out that there are many difficulties. The eyelid response to sound is considered an adjunct test in certain cases, whether conditioned (56) or not (1, 97).

With Kohler's old technique of tiny mirrors affixed to the eardrum, Kobrak (90) provides an optical enlargement of eardrum rotation to sound. An audiogram may in some cases thus be had. Mendelson (114) measures the meatal pressure change upon stimulation of the contralateral ear. Bennett (17) attaches a fine blunt wire to a vibrator and places it directly

on the eardrum at a variety of places to assess middle ear function. Anderson *et al.* (3) propose a new type of datum, the lag in phase between the vibration of the skull as a unit and the vibration of the ossicles, which of course are normally only loosely attached to the skull. Sound pressure level is recorded from a probe tube microphone fitted into the meatus. When the drum has lost its normal movements due to middle ear lesions, or when the intratympanic muscles are contracted, the sound pressure level is increased over the normal.

Maraschi & Casorati (109) note that an audiogram can be obtained using alternating current through the head. They even suggest that less than usual distortion may be involved. Finally, Benciolini & Fraccaroli (16) report good success in adults with the simplest of conditioning techniques involving simultaneous sound and a flickering red light.

METHODOLOGY IN THE AUDIOLOGY CLINIC

Schröder (144) has provided considerable space in a long monograph for a full discussion of the relation between classic psychophysics since Fechner, and clinical tests of hearing. He goes far beyond the usual insistence that threshold audiometry is merely the method of limits applied to the absolute intensive limen. This and similar articles should go far to insure that experimental psychology be applied correctly in the audiological clinic.

Several papers have appeared relating hearing for speech to hearing for pure tones (66, 82, 123, 137, 156). The issues here are: what frequencies contribute to speech intelligibility, and what clinical significance attaches to those ears with marked discrepancies.

The use of very brief tones in audiometry is only now receiving attention (129). Miskolczy-Fodor (115) and Miskolczy-Fodor & Simor (117) have added new information to the former's pioneer study. Harris (63) elucidated the relative importance in very brief tones of total energy, total utilizable energy, peak energy, total peak energy, and rise-fall time (stimulus envelope). Palva (128) has shown that tones presented with a 50 per cent duty cycle will yield thresholds equal to continuous tones, so long as the individual spurt-length is long enough for the tones to achieve maximum loudness.

Webster *et al.* (171) find that for assessing ear plugs it is as well to use narrow bands of noise instead of pure tones.

Discussion during the past year was full on the question of reported differences between "Normal Hearing" for American audiometers and those of other countries. Most of this material is still to appear. Harris (65) investigated certain possible reasons for discrepancies—it seems, for example, that the same sound pressure level at the entrance to the ear canal may not always yield equal loudness if the two phones to be compared have markedly different cushions.

The detection of simulated deafness is always a problem. Bocca (20) proposes an entirely new test for unilateral simulation based upon the fact

that the patient will not be aware that intelligibility for speech led alternately to the two ears at certain alternation rates will deteriorate rapidly if true unilateral deafness is present.

Lawrence & Yantis (99, 100) have continued to add data to their studies of aural harmonics in the overloaded ear and the pathology and relation to fatigue susceptibility of these harmonics.

Butler & Albrite (22) have studied the deterioration of pitch discrimination by the constants method in the perceptively deaf ear. Schubert has devised an apparently very delicate test of hearing, the deterioration in speech reception when the recorded test is speech taped with a 5 sec. reverberation time (145).

AFTER-EFFECTS OF ACOUSTIC STIMULATION

The course of recovery has been studied again (44, 80). Eldredge *et al.* (42) continue their microphonic and histological studies on the guinea pig exposed to very high levels.

Many workers have proposed a relatively brief fatigue test to screen damage-susceptible individuals (33, 81). Many have sought to know why only certain ears are affected. Eliasson & Gisselsson (43) note that none of the intratympanic reflexes could be observed if the cat had or *had had* (italics the reviewer's) otitis media. Takahashi (153) destroyed these reflexes in the rabbit with drugs, and gave fatiguing noise, with quite a marked effect, which in a control experiment (154) was repeated when the tensor reflex was abolished by trigeminal nerve section.

Jankowski & Iwankiewicz (76) gave fliers 4000 c.p.s., at 80 db for 5 min. at sea level, with complete recovery within 20 sec.; at 6500 m., this same sound caused a loss of 27.6 db rather than 10 db, and after 3 min. the threshold was still elevated by 10 db. We are certainly dealing here with a new and very important principle, the multiplicative combination of etiological factors. Mizukoshi *et al.* (118) found approximately the same thing on guinea pigs, combining fatiguing stimuli with quinine and dihydrostreptomycin. Even aspirin can in massive doses drastically reduce threshold (170); one should be alert for additive effects of noise and certain drugs.

Mizukoshi *et al.* further show an analogue of "sensitization" in psychoacoustics. Hughes & Rosenblith (71) found a similar analogue in the first neural event, N_1 . It would seem that their explanation that sensitization is a neural phenomenon allied to post-tetanic potentiation can hardly be correct if the precursor exists in the preneural event.

Two groups have suggested that pneumatization of the mastoid helps avoid auditory fatigue (27, 75). We may well be looking here at an important factor in this puzzling field. It is helpful to consider Tumarkin's monograph (166) on the comparative anatomy and pathology of the accessory spaces of the middle ear, and Legoux & Wisner's paper (102) on the comparative anatomy of the bulla.

PERSTIMULATORY ADAPTATION

Hood (70) adds to his body of fact and interpretation on decreases in loudness occurring during continuous stimulation. Jerger (79) provides full data for a variety of frequencies, loudnesses, and durations of stimulation. When adaptation was expressed in sones, frequency differences largely disappeared. Carterette (24) extended such observations to broad and narrow bands of noise. When one uses critical band theory, a good approximation to the noise data can be predicted from the pure tone data.

RECRUITMENT

Recruitment is important here since data from the pathologically defective ear may throw light upon the nature of the hearing process in the normal ear. Several authors (77, 148) reiterate that true nerve lesions do not produce recruitment [but see Thiébaud *et al.* (161) for secondary effects]. The relation between loudness balancing and the DL for amplitude modulation has been explored by Hedgecock (67) and by Kawabe (83) who gives complete data for 73 patients. Absolute values for the DL either by amplitude modulation or by a constants method (5) cannot be easily related to recruitment, but in the modulation DL only, a recruiting ear will suffer much less than normal as the tones are made weaker. Harris (64) has sought to explain this and other similar seeming inconsistencies by showing that different clinical tests, supposedly measuring the same thing, are actually sampling entirely different auditory traits.

Krejci & Fischer (95) show that about 90 per cent of cases of presbycusis (which generally does not yield recruitment) show an amplitude modulation DL of 8 per cent or more, in some cases up to 70 per cent modulation. Most normal ears can easily detect an 8 per cent modulation. We have here a most interesting seeming discrepancy.

The most informed paper on the relation between tests proposed for recruitment is that by Palva (127), who gives data for 33 patients with nonconductive lesions classified by Harris' scheme according to shape of recruitment curve. Each of several tests was shown to have its uncertainty, but Palva stopped just short of pointing out how the interrelationships among all of these tests might assist materially in diagnosis.

NORMAL ABSOLUTE AND DIFFERENTIAL THRESHOLDS

Absolute frequency threshold.—Huruyama (72) has used carefully controlled tuning forks to determine the lower limit of tone perception to be about 40 c.p.s. This is a bit higher than is usually found.

Interaural effects on absolute intensive threshold.—Miskolczy-Fodor (116) has added the principle of interaural listening to the matter of acuity testing with one or two ears. On nine normal-hearing individuals with slight binaural differences in acuity equated electrically, an improvement of 2.93 db was found for the interaural over the monaural condition. This agrees

well with the simultaneous-presentation binaural technique. Ingham (73) studied the monaural threshold for 1000 c.p.s. pips when the other ear was being stimulated with 400 c.p.s. continuously at the 10 or 30 db sensation level. Attention, practice, fatigue, etc., were controlled with instructions and experimental design. There seems to be an incontrovertible deleterious effect of the 400 c.p.s. tone. The author suggests there may be a direct inhibitory influence of the central nervous system on the ear, as has been demonstrated in the retina and muscle spindle. The "competing stimulus" theory is untenable because when attention is directed to the right ear but no stimulus led to that ear, the left ear threshold does not suffer.

Thresholds for noise bursts.—Thurlow & Bowman (162) and Mobray *et al.* (119) offer an extension of the work of Garner and of Pollack on the absolute intensive threshold for noise as a function of burst duration and repetition rate. Perfect energy integration is not quite achieved, but the nature of the summation which does occur is as yet speculative. Feldtkeller & Oetinger (47) studied temporal summation for short bursts of noise, for even shorter gaussian pulses of noise at durations of 5 msec. and less, and for short bursts of pure tone, both in quiet and with a masking noise. Rather complete data are provided for frequencies from 250 to 4000 c.p.s., levels of masking up to 80 db, and durations from less than 1 msec. to 1 sec. In general, masked tone thresholds follow unmasked thresholds at all frequencies and masking levels in requiring a 10 db increase in intensity to compensate for shortening the duration by 1 log unit. In their linear portions, curves for short gaussian pulses (up to 5 msec. duration) also follow this equation, whether masked or not; but for square-wave bursts of noise of longer duration, the constant 10 drops to about 8 db. The significance of this is discussed in terms of the effective band width of the stimulating spectra.

A very full treatment of the loudness of short gaussian pulses and of short exponential pulses is given by Bauch (6, 7). The amplitude spectra are analyzed, and loudness matches provided against a 1000 c.p.s. standard tone. Experimental and calculated phon values are given, for example, for gaussian pulses at pulse frequencies between 2.5 and 8000 c.p.s., with rise-times from .05 msec. to 4 msec., and at a variety of peak loudnesses. Considerably better prediction was achieved with exponential pulses.

Annoyance and loudness of complex sounds.—Spieth (147) has shown that all of 13 different bands of noise from 50 to 13,000 c.p.s. are about equally annoying, the annoyance value varying as expected with work experience.

An extremely careful and extensive study of equal-loudness contours has been published by the British equivalent of the United States Bureau of Standards (141). These data should shortly begin to appear in handbooks and textbooks as the standard for free-field isophonic contours. Quietsch (136) made loudness matches with a 1000 c.p.s. tone or with a 900 to 1120 c.p.s. band of noise against 37 widely differing samples of noise, while Zwicker & Feldtkeller (178) wrote a new set of equal-loudness contours and a new loudness scale, and made use of these data in predicting the loudness of

wide-band noise. These papers deserve a full reporting, but fortunately they are considered extensively in Stevens' compendium paper on the loudness of complex noises (150). In this paper Stevens gives a complete discussion of the background of attempts to compute the loudness of sounds from sound pressure level measurements of certain band widths. He provides a nomograph and chart to estimate the loudness in sonos of an octave-band of noise knowing only the sound pressure level of that band. In case of a band of noise comprising more than one octave, Stevens provides a formula for the total loudness from the physical measurements on each band. As an added convenience, the formula can be altered slightly to fit physical data measured in half- and third-octave bandwidths. It seems likely that Stevens' data and concepts will not be superseded in the near future by more definitive experiments or clearer thought.

Differential intensity sensitivity.—Chocholle (31) gives complete psychometric functions for two very experienced subjects asked to judge whether they heard an almost instantaneous increase in the intensity of a pure tone. Curves of DL vs. sensation level on four subjects are given at 200, 1000, and 10,000 c.p.s. Little or no variation with frequency was found, but the DL varied from about 3 db at the very weakest intensity to about 0.5 db and less at medium to high loudnesses (40 to 100 sensation level). This is in line with previous studies using any stimulus method involving a quick transition from one intensity level to another, but not in line with studies utilizing an interval of silence between stimuli to be judged. Evidently one is dealing with factorially different auditory traits.

Oldfield (126) studied the continuous mechanical recording of the tracking behavior of a subject as he caused a motor-driven attenuator to make more or less detectable the amplitude modulation of a tone. In this way, momentary fluctuations of differential sensitivity can be studied if present. Oldfield found no regular periodicities of sensitivity in his subjects, and he ascribed occasional fluctuations to the personality domain rather than to the auditory domain. An almost identical technique has been presented (122) except that a series of short tones was presented rather than amplitude modulation.

Pollack (133, 134) has shown the correspondence between the absolute judgment experiment and the discrimination experiment. The complexity of the set of possible stimuli is the unifying concept, and the ease with which the listener can refer to that set in making the identification, reproduction, or discrimination, whichever is called for.

Differential frequency sensitivity.—Cramer & Zeitlin (37) and Zeitlin & Cramer (177) have considered complex tones. Flanagan & Saslow (51) have added some most interesting data on the very slight change in the fundamental frequency which is detectable with synthetic vowels. A change in the first formant of less than a cycle is sufficient for discrimination. Much research remains to be done, but if this finding is extended to true harmonic arrays we are here dealing with a phase-sensitive system.

Tanner (157) extends his concepts of signal detection in noise to the problem of the recognition of shades of sensory quality. In pitch discrimination, for example, the observer is assumed to be a narrow-band receiver, with a probability response curve at a certain moment such that, for example, 900 c.p.s. will be at the center of his attention span, but 1000 c.p.s. will be beyond it. In the other case, the same is assumed for 1000 c.p.s. Thus, the attention curves for 900 and 1000 c.p.s. are assumed to be independent, and joint probability curves can be constructed. But, if the two attention systems overlaid one another they are not independent but correlated, and the extent of the correlation must be estimated. Two new constructs are proposed, d' (a standard score index of sensitivity), and θ (an estimate of the correlation between the hypothetical attention curves).

It is far from the reviewer's wish to give any helpful concept an unsympathetic treatment, but the marriage of rather complicated algebra with almost incredibly naive assumptions about the nature of biological systems has a familiar ring in audition, where it has often been tried and found wanting. In this paper, for example, one finds such statements as, "The assumption . . . is that the observer behaves as a mathematically perfect device." Would it were so. And would that we had empirical evidence for the shape of the attention curves. The reviewer has the feeling that there may be an important clue to the nature of discrimination buried in Tanner's impressive paper, but that since all the constructs arise from time-worn notions of probability it is more likely that the matter is one of reformulating ideas expressed very clearly by Titchener in 1904, but now cast in the language of the radar detection laboratory.

Time and phase effects.—When a sound is led in phase to the two eardrums, a midline localization is normally obtained. When, however, a binaural phase difference is introduced, the apparent source tends to move to the side of phase lead. Zwislocki & Feldman (180) performed this experiment with a selection of frequencies and intensities, summarizing their results in a handy three-dimensional chart. Phase sensitivity is lost at about 1200 to 1500 c.p.s. but can reach the quite small value of 2° for 230 c.p.s. at 70 db sensation level. The just noticeable difference in time, calculated at equal phase, reaches a minimum of 12 μ sec. for 1000 c.p.s. at 65 db sensation level, but drops off rather sharply at adjacent frequencies. Klumpp & Eady (88) studied sensible differences in time of arrival at the two ears for a band of noise, a 1000 c.p.s. tone, and a click. Thresholds were, respectively, 9, 11, and 28 μ sec. The organization of the nervous system capable of differentiating small units of microtime, almost without parallel in the biological world, and far shorter than the time required for the all-or-none nerve response of a single neurone, is truly astounding. The close correspondence between these two studies and a third from Jeffress' laboratory (19) on the interaural time difference at which sidedness is lost is obvious. It is to be hoped that theoretical papers will shortly appear relating these and similar experiments to the neurology of interaural and binaural hearing.

Masking and contact detection.—This area is being studied today by many fine laboratories; quite a number of papers and an important symposium are in press. It seems best to defer for one year a full discussion of this topic until new data are in with which one can revise the classic concepts of Wegel and Lane. We will, therefore, simply list a few of the most important papers as a guide.

Tanner's laboratory (61) has revitalized the field of contact detection in noise, providing data with a variety of parameters. Lugli *et al.* (107) confirmed the Harris-Egan effect, as Thwing (163) calls it, that masking does not much decrease with continuation of the masking stimulus, but Thwing shows that if the duration of the masked stimulus is long enough for perstimulatory adaptation, then a reduction of the necessary masking energy may be expected. Jeffress *et al.* (78) give a fine paper on monaural-binaural neurological models. Fairbanks *et al.* (46) show the effect of attitude of the observer. Lightfoot *et al.* (104) show the effects of hearing impairment on masking. Bilger & Hirsh (18) use the Békésy audiometer to study masking of tones by noise, and Deatherage *et al.* (39) give a physiological clue to some of the masking of low tones by high tones. Licklider & Guttman (103) used a variety of pure tone combinations to mask speech, and the paper should be mentioned here even though this review specifically excludes the speech area.

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PHYSIOLOGICAL PSYCHOLOGY¹

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The books and reprints which served as a basis for this review were largely limited to materials written in English. This fact is regrettable on several accounts. For one, this reviewer had a fine time perusing a Latin work by Georgius Prochaska (1), ancient (1749–1820) but illustrious *Landsmann* who has earned a place in the history of science by his theory of nervous function. A hundred years ago his ideas attracted a good deal of attention in England in connection with the controversy over theories of reflex action, and a selection from his writings (2) was included in Fulton's anthology. However, for a full story of Prochaska's life and accomplishment one has to consult a monograph which is available so far only in Czech (3).

Tough-minded physiological psychologists may regard excursions into history, even when they involve less esoteric media of communication than Latin or Czech, as sheer waste of time or as a luxury at best. However, a study of the filiation of important, fertile ideas is more than an innocuous pastime. While the detailed evidence is still lacking, it appears that Prochaska, through the Russian translation of his textbook of physiology, may have influenced the reflex theory of I. M. Sečenov (*Reflexes of the Brain*, in Russian, 1876) and, indirectly, Pavlov and his school. Pavlov's work (4) and the recent developments in Russian physiology of the higher nervous functions (5) continue to be of interest. At the same time, the effects of "pavlovism" as a dogma, as a Procrustean bed—very short, very narrow, and very hard—on which scientific psychology, theoretical and applied, was laid in the Soviet Union and in the countries within the Soviet orbit, have been unfortunate. This has been made very clear by a Polish author (6) who shows the disastrous consequences of a combination of pressures, including the "physiologizing" of psychology.

REVIEWS AND GENERAL TREATISES

Some recent reviews not referred to in the body of this survey may be registered (6 to 10). In a field in which textbook writing is not a favorite hobby, a new introductory text deserves notice (11). The wares are labelled physiological psychology or psychophysiology (psychobiology), without distinction. While there is some vacillation in regard to the flag under which the authors sail, their basic point of view is quite definite: the study of behavior has vital roots in physiological sciences. Admittedly, sampling of the texts used in the majority of introductory courses in psychology would

¹ The survey of the literature pertaining to this review was completed in June, 1957.

not reinforce this thesis. But this reviewer shares the conviction that training in psychology demands an early acquaintance with physiological terminology, methods, factual data, and theoretical interpretations relevant for the study of human and animal behavior.

Herrick (12), writing from the awesome perspective of close to 60 years of creative research in the comparative anatomy of the nervous system, has given us his version of psychobiology. His goal is not to instruct a captive audience of students, registered for a college course, but to trace the natural history of human nature and its biological origins, with special reference to the structure and the physiological properties of the cortical integrative apparatus, in the hope that a synthesis of the available knowledge on the general biological and neurological factors in behavior will be useful for the guidance of human conduct.

NERVOUS SYSTEM

Recent advances in our understanding of the organization and function of the nervous system were considered competently and in detail in the preceding volumes of the *Annual Review of Psychology*. For lack of space and because of interest in other topics, this presentation will be limited to a brief comment on nonperiodical publications that came to the reviewer's attention.

In the field of interdisciplinary brain research we have a progress report (13) on the explorations of the potentialities of psychosurgical methods as a therapy for mental disorders and of the role of the frontal lobes of the brain in human behavior. The two types of partial ablation (topectomy) that were selected for study consisted of a bilateral removal of the superior or orbital surfaces of the prefrontal lobes. While the study of a common group of patients from the viewpoints and with the techniques of surgery, physiology, psychophysiology, psychometrics, and psychiatry has enriched the fund of empirical data, it was felt that much remains to be learned before the formulation of a comprehensive theory of frontal lobe function can be attempted. In the psychophysiological area visual, auditory, somesthetic-kinesthetic, and psychomotor functions were studied. Only temporary effects of the operations in some of these functions were observed. In a volume which covers the theoretical, experimental, and therapeutic aspects of hypnosis, a chapter was devoted to the physiological effects (14).

Among works largely peripheral to the interests of physiological psychologists may be cited Sholl's presentation of the organization of the cerebral cortex (15); Eccles' (16) account of explorations based on the use of electron-microscopy and microelectrodes for the recording of intracellular and extracellular potentials, with special reference to pathways and synaptic transmitter substances in the central nervous system; the proceedings of the first international meeting of neurobiologists, combining the neuroanatomical and the neurophysiological approaches (17); the re-issue of McDowall's monograph (18), with a supplemental volume (19) which includes Kety's

chapter on cerebral circulation (pp. 176-83); the consideration of the problem of how physical stimuli are transduced, at the molecular level, into nervous excitation (20); and a symposium on the chemistry of the nervous system (21).

HUNGER

Injections of amphetamine derivatives, which in unanesthetized cats depress food intake, produce changes in the electrical activity (an increase in frequency and amplitude) recorded from the medial hypothalamus (22). This fits in with the earlier work in which it was postulated that the ventromedial region of the hypothalamus contains the inhibitory part of a feeding center. Mayer (23) stressed the specificity of goldthioglucose in causing ventromedial lesions in mice who eventually became obese.

Anliker & Mayer (24) described in detail a free operant conditioning technique, designed for a fine-grained analysis of feeding behavior. The animal releases small pellets of food by depressing a lever. The activation of the lever is recorded and the cumulative frequency curves provide a permanent record of the temporal feeding patterns. The method served to bring out (25) large differences in the feeding rates of hyperphagic (goldthioglucose, hypothalamic, hereditary obese) and normal mice. It is postulated that the satiety center obtains information from the bloodstream via glucoreceptors. The destruction of the satiety center acts to release the feeding center from inhibitory influences and results in hyperphagia.

Several investigations were concerned with the validity of the glucostatic theory. Bernstein & Grossman (26) believe that the results of their experiments constitute crucial evidence against the glucostatic hypothesis, with variations in blood glucose levels within the range studied playing no important role in the regulation of food intake in young normal adult males. Stunkard & Wolff (27) approached the problem through the measurement of gastric contractions as an indicator of hunger. They interpret their findings as supporting the view that metabolic events accompanying increased peripheral removal of glucose have an inhibiting effect on gastric hunger contractions. On the other hand, events associated with decreased removal are not a sufficient stimulus for the production of gastric hunger contractions, although they appear to be a necessary precondition.

Effects of intravenous administration of various nutrients were studied (28) in a young, underweight man severely injured in an accident and presenting the picture of a decorticate man. Following the injection of fat, amino acids, and glucose the hunger contractions persisted for hours without interruption. Intravenous administration of glucagon, the hyperglycemic-glucogenolytic factor of the pancreas, resulted in a small rise in the capillary and venous levels and, more importantly, a significantly enlarged capillary-venous glucose difference indicative of an increased glucose removal (utilization). Gastric contractions were promptly abolished. The findings are considered as supporting the glucostatic (Mayer) rather than the thermostatic

(Brobeck) theory of food regulation. Unexplained remains the return of gastric contractions while the uptake of glucose, as seen peripherally, was still high.

In considering the broad problem of the regulation of food intake, the factors accounting for the cessation of eating deserve as much (or more, in our culture) attention as the initiation of eating. Siegel (29) summarized some of his recent work on the problem of monotony as it is found in the human eating situation.

THIRST

The new apparatus for measuring drinking in small animals yields accurate records of intake over prolonged periods (30). The drinking behavior in normal satiated rats can be controlled by means of an operant-conditioning technique (31), combining the registration of drinking responses by means of an electronic "drinkometer" with the application of intermittent shocks delivered through the grid floor of a cage. By drinking the animal can postpone the next shock. Using appropriate schedules of reinforcement it is possible to obtain ingestion of liquids that are normally refused and very large intakes of fluids. When a liquid nutrient is offered, forced ingestion results in marked obesity.

Nonthirsty rats prefer tap water to distilled water (32). Under thirst conditions (20 to 23 hr. of water deprivation) the rats were more active, making more runs per minute between the starting box and the glass cups containing the two fluids, but were less prone to discriminate preferentially between the test fluids. The authors interpret their findings in terms of increased acceptability of all fluids. At the higher appetitive level the difference between the two fluids is below the differential threshold.

At lower concentrations (below 2 per cent NaCl) nonthirsty rats prefer salt solutions to distilled water (33). While these rats occasionally accept salt solutions of 3 per cent and 6 per cent, the thirsty animals consistently reject the hypertonic fluids, preferring water. Thus thirst is associated with a shift in palatability.

Several reports were concerned with the details of the nervous control of water consumption. In the rat, small lesions in the lateral hypothalamic areas produced little changes in food consumption (34) but a definite reduction in water intake. Other lesions (especially those located in the middle and caudal hypothalamus) produced relative hypodipsia, the low water/food intake ratio being due to a dramatic increase in food intake without any significant decrease in water consumption. These thalamic lesions, while never resulting in a complete absence of voluntary water intake, disturb the regulation of water metabolism. The hypothalamic hypodipsic rats fail to regulate their water intake in reference to the amount of food eaten and do not increase their water when transferred to an environment with elevated temperature.

Isolation of the frontal pole of the brain in dogs did not reduce the water

intake or affect the compensatory drinking response to intravenous injections of strongly hypertonic saline (35). This would indicate that the destruction of the connections between the frontal lobes and the hypothalamus does not affect the regulation of water and electrolyte metabolism. However, studies utilizing intramuscular injections of amphetamine indicate that, while in normal dogs the drug completely inhibits the drinking of water after an intravenous injection of 20 cc. of a 20 per cent NaCl solution, the inhibitory effect of amphetamine is diminished or abolished following prefrontal lobotomy. The inhibitory effect, present in unoperated animals, must be at least partially exerted from structures localized in the prefrontal areas of the cerebral cortex.

In their search for the cortical representation of the thirst sensations, Andersson & Larsson (36) attempted to develop a conditioned drinking response, using electrical stimulation of the drinking area of the hypothalamus of the goat, with its clearcut polydipsic effect, as the unconditioned stimulus. The neutral stimuli, presented together with the electrical stimulation, consisted of a light or a tone. However, the attempts to obtain a conditioned drinking response to the neutral stimuli proved completely fruitless. The negative results of the study are compatible with the absence of evidence for the existence of stimulatory fibers running from other parts of the brain to the hypothalamic drinking center. The hypothalamus appears to be the only part of the nervous system from which drinking can be elicited and there is nothing to indicate that the feeling of thirst has special cortical localization.

TASTE

Advances in electrophysiological techniques have created means for the basic study of taste in terms of the electrical activity within the taste nerve fibers. Some of the recent advances were reviewed by Zotterman (37). In an important study Pfaffmann (38) obtained electrophysiological evidence on taste sensitivity in the form of integrated action potentials which represent afferent nerve impulses originating in the tongue and carried in the taste fibers. The recording electrodes were placed at the chorda tympani nerve up which most of the taste fibers pass. The consumption of a particular solution, as compared with intake of water, constituted the behavioral response. Responses to solutions varying in the concentration of quinine, hydrochloric acid, sodium chloride, and sucrose were studied. The adrenalectomized, salt-needy animal shows a preferential response for salt solutions at all concentrations. At the same time, the electrophysiological threshold for salt remains unaltered. Consequently, the altered behavior reflects not a change in the peripheral afferent neural message but in its significance for central neural processes.

In reference to the mechanisms regulating food intake, three viewpoints have been expressed in the literature, emphasizing: (a) the role of taste of foods and sensations of satiety, (b) sensations experienced after ingestion

of food stuffs involving a conditioning process, and (c) the need for a rational choice of components and amounts based on objective criteria (39). Here we are concerned with appetite, not with the general hunger drive. P. T. Young (40) defines appetite (a specific hunger) as a determination to seek out and select a particular nutrient. In addition to complex physiological regulators, including the chemical state of the organisms as determined by its constitution and dietary history, food intake depends upon the palatability of the foods, the affective arousal that follows the ingestion of certain foods or their absence in the diet, and existing habits or attitudes. A combination of a chemical and a psychological approach is considered a prerequisite for an adequate analysis of patterns of food preferences and addictions.

The methods for the study of food acceptance in man were surveyed by Pilgrim (41) in reference to criteria of acceptance (consumption with pleasure), components of food acceptance (physiological, sensory, attitudinal), and technique of measurement (attitude studies, affective and discriminatory sensory tests, and food consumption). A profile method of flavor analysis, combined in A.D. Little Flavor Laboratory with physicochemical studies designed to isolate and identify the crucial flavor components, was described in detail by Caul (42). At times only a single, narrow aspect of flavor, such as the bitterness of orange juice (43), is of interest.

While cultural anthropologists and physiological psychologists do not mix very frequently, Lee's (44) discussion of social and cultural modifiers should not pass unnoticed. Culture determines, in part, what will whet the appetite or bring a feeling of satiety. Even what is recognized as food (including the tasty, crisp-fried maguey worms of Mexico) depends on culture (45).

Torrance & Mason (46) reported three studies on factors affecting the acceptability of pemmican, a meat product recommended for Arctic use, in realistically simulated survival situations. Low acceptance was associated, in addition to incidental factors, e.g., unfavorable conditions at the time of initial use, with such specific factors as history of food aversions and such general personality characteristics as weak ego-strength and relatively unaggressive adjustment to life in general.

Griffiths (47) investigated the nutrient selection of domestic Norway rats subjected to the stress of treadmill running. Out of fifteen substances offered in solution, five were selected in different amounts during the periods of stress. While the biological significance of the altered pattern of nutrient selection is not altogether clear, it is of interest that the largest increase took place in the consumption of calcium pantothenate, with average amounts of 2 ml. and 9 ml. of the solution ingested prior and during treadmill running.

There are large differences among rats in regard to voluntary alcohol consumption when the animals are allowed a choice between dilute alcohol

and water. No significant differences were obtained in the rates with which alcohol is oxidized by the drinkers and the nondrinkers (48).

NUTRITION

The relationships between nutrition and behavior are complex. As such, the problems are best studied in an interdisciplinary setting in which the psychologist contributes techniques for a quantitative study of behavior in man and animals. However, the coat has another side with a rich design. There is a broad spectrum of psychological problems, from the dark-adaptation of airplane pilots to temperamental differences among certain population groups, in which the nutritional factors may be important. MacLeod (49) brought out two problem areas as particularly profitable fields of collaborative studies: the motivation of behavior, and capacity for performance.

Food is a basic need, more basic than sex. But food behavior, to continue with MacLeod, involves more than the satisfaction of a simple organismic need. It includes appetites, esthetic enjoyments and remote goals, including longevity itself, extending over the whole range of human motivation.

The practical challenge is obvious: how can we through dietary regulation improve the lot of man? Behind this practical matter, however, are a multitude of scientific questions. What are the effects on human behavior of restricted caloric intake? of protein deficiency? of deficiencies in specific vitamins and minerals? Can human capacities be improved through dietary control? What is the relation between diet and temperament, or between one's diet and one's philosophy of life? The challenges to scientific curiosity seem limitless (49, p. 108).

We may study (50) the somatopsychological sequence of events (consumption of a diet deficient in niacin and tryptophan, alterations in the *milieu intérieur*, deranged metabolism and function of central nervous system, and the psychotic behavior of a pellagrin) or the psychosomatic relations (in a patient with anorexia nervosa, starving himself, perhaps to death, in the midst of an abundance of food). The effects of food on behavior, and the attitudes—the behavior of the organism toward food—represent two facets of a larger problem (51).

In order to obtain an up-to-date picture of the geography of this border area, a symposium was organized with the cooperation of the National Vitamin Foundation, Inc., New York, N.Y., and held on April 27, 1956 at the Laboratory of Physiological Hygiene, University of Minnesota. The proceedings (52), together with an epilogue (53), were published in the *American Journal of Clinical Nutrition* and reprinted (54) in book form.

Studies on animals.—Animal behavior has a number of parameters that are of interest in nutritional research, in addition to food consumption.

Spector & Young (55) provided a critical review of methods for the measurement of physical performance capacity. Three types of methods were

considered: artificial stimulation of perfused muscles, measurements of voluntary activity, and the study of forced activity of the whole animal. The last type of activity, in the form of running or swimming, is considered particularly promising.

In experimental studies on man, the intellectual functions are surprisingly resistant to nutritional stresses. In dogs, subjected to diets deficient in vitamins on the B-complex (pantothenic acid, pyridoxine), disturbances in the conditional reflexes manifested themselves after four to fifteen days of maintenance on the deficient diets and became progressively worse (56). The disturbance consisted in the failure to differentiate between tones of different pitch, notwithstanding repeated practice (the excitatory conditional stimulus being followed by a faradic shock to the foreleg). At this time there were no observable changes in overt behavior or in unconditional reflexes. Return to adequate diet restored differentiation. The method of conditioned reflexes has not been fully utilized in studies on the relations between nutrition and nervous system.

Weiss (57) used a modified Skinner box with his rats maintained in a cold (0° C.) environment. By pressing a lever the animals activated a heat lamp. When placed on a diet deficient in pantothenic acid, and subsequently submitted to starvation, the experimental animals were more sensitive to cold than the controls deprived only of food, as indicated by a significantly greater increase in the rate of lever pressing.

Experimental studies in man.—Methods used in experimental studies on diet and behavior vary according to the metabolic and physiological role of the nutrient, the purpose of the study (applied—testing differences between levels of intake—versus fundamental concern with the biological significance of a nutrient), and the facilities at the investigator's disposal. The principal feature distinguishing nutritional from other types of research in psych chemistry is the fact that the period of observation extends not over minutes or hours but weeks and months, sometimes years.

Psychological changes in acute starvation, combined with hard physical work (58), were examined in a series of investigations on physiological stresses interposed in the course of a study on thiamine requirements. While tests of intellectual functions showed little or no impairment after four days of starvation, statistically highly significant deterioration occurred in tests of speed of movements and in tests of eye-hand coordination. Tiredness, feelings of muscular weakness (with no counterpart in strength measurements), and muscle soreness dominated the subjective symptoms.

The impact of partial restriction and of acute thiamine deprivation was studied in reference to sensory and intellectual functions, motor performance, and personality (59). The study, utilizing behavioral as well as biochemical criteria (60), contributes to our information on the thiamine requirements of normal young men. More importantly, the study provides a comprehensive characterization of the changes, some of them rapid and profound (personality, some motor functions), resulting from maintenance on thi-

amine-free diets. The effects of supplementation of the experimental diet with synthetic vitamin were striking both in the speed and the degree of recovery.

Pantothenic acid deficiency syndrome, induced by maintaining three normal human volunteers on a diet devoid of pantothenic acid or containing a metabolic antagonist (omega-methylpantothenic acid), included such behavioral and sensory changes as profound apathy and depression and a neuromotor disorder with paresthesias (numbness and tingling of the extremities, burning sensations in feet) and complaints of clumsiness, dizziness and weakness. The men, cheerful at the outset, became quarrelsome, sullen, and petulant (61). The symptoms were reproduced on extending the observations to additional subjects (62). It is a pity that no quantitative data were obtained on the emotional, intellectual, or psychomotor functions. Similarly, in metabolically oriented studies on niacin deficiency (63), depression and apathy were noted frequently as a part of the deficiency syndrome and in the absence of demonstrable neurological changes, but no quantification of behavioral response was attempted.

Clinical and field studies.—Nutritional deficiency of nicotinic acid (64, p. 133 f.) may result in a genuine psychosis. In phenylpyruvic oligophrenia, a rare form of mental deficiency, we deal not with a dietary deficiency but with a faulty protein metabolism, a failure normally to metabolize phenylalanine, a dietary amino acid. An excess of this substance accumulates in body fluids. It is converted, in part, into phenylpyruvic acid which the patients are also unable to oxidize and which is excreted in the urine. By maintaining children with phenylpyruvic oligophrenia on diets low in phenylalanine, substantial improvement in mental functioning has been obtained (65, 66, 67). Several authors have stressed that phenylalanine-restricted diets should be initiated at a very early age in order to prevent irreversible damage to the central nervous system (C.N.S.).

The work of Bickel (68) and others was based on the assumption that the accumulation of phenylalanine or some of its breakdown products in blood and tissues was the direct cause of damage to the C.N.S. and of the resulting mental deficiency. However, the real cause of the mental deficiency in phenylketonuria remains obscure. It appears that the urinary excretion of phenylpyruvic acid is not unconditionally associated with mental deficiency (69).

Glutamic acid is another amino acid that in the last ten years has received a good deal of attention in reference to nervous and mental function. It was studied as an anticonvulsant, in relation to its ability to restore consciousness when administered to hypoglycemic subjects and, with equivocal results, in mentally defective subjects (64, p. 111). The literature is fairly large and has not been, as far as we are aware, critically summarized.

Results of a large-scale study on the effects of mothers' diets on measured intelligence of the offspring were reported by Harrell *et al.* (70, 71).

The difference between the extremes (placebo and polynutrient sample) was statistically significant, a trend confirmed in subsequent retesting, but relative superiority of the three types of supplements—varying markedly in relevance to the development and function of the C.N.S.—could not be demonstrated statistically.

Psychological changes associated with severe protein malnutrition in children (kwashiorkor) are being studied at Kampala, Uganda (72). For 25 children the behavioral status at three stages of recovery was described in qualitative terms and in reference to results obtained on the Gesell tests. As a part of the background information, the psychomotor development of normal African children was studied (73).

Dhanda (74, 75) used electroretinography for the study of the effects of vitamin A deficiency. A certain level of vitamin A in the blood is required for the normal functioning of the rods of the retina (76). A normal, dark-adapted eye stimulated by a flash of light yields a characteristic action-potential record. The electroretinogram is extinguished when the vitamin A content of blood falls to a level that is associated with clinical manifestations of night blindness. The electrical response is reestablished when vitamin A level rises in the course of therapy. In this connection the report of a new method for the study of dark adaptation in the pigeon may be of interest (77).

Nutrition is closely associated with some aspects of endocrine function (78). The classical example is the role of dietary iodine intake in the production of thyroid hormone which, in turn, controls the rate of energy metabolism (cellular oxidation) and influences physical growth and neuromuscular functioning. In man, hypothyroidism of the cretin tends to be associated with retarded mental development in the young and mental dullness and apathy in both the young and the old (79, p. 284). However, impaired intelligence is not a concomitant of acquired hypothyroidism (80).

STRESSES

If the invited address (81) presented at the American Psychological Association Day—the climax of annual meetings—may be taken as a signpost, 1956 was The Year of Stress. However, stress means different things to different investigators and its study will be approached in varied ways, both in the somatopsychological and the psychosomatic direction. Strictly psychological studies on stress will not be considered here.

As late as 1956, Selye, who has written more about stress than any other individual, felt the need for a clear definition in such a compelling manner that he has published another paper on this very subject (82). To Selye stress refers to bodily changes, differentiated from the factors which produce these changes and to which he refers as stressors. Stress is the common denominator of all adaptive reactions in the body. There are many things with which stress, in Selye's usage, cannot be identified: it is not nervous tension, not an emergency discharge of hormones from the adrenal medulla,

not any deviation from homeostasis. His emphasis is on the endocrine regulators of general stress reactions, particularly the pituitary and the adrenals. The interaction between the nervous system and the humoral responses to stress has been considered by Selye in another publication [(83, pp. 41-82); cf. also (84, 85, 86)].

To Shock (87, p. 44) physiological stress means the application of a measurable physiological stimulus, with quantitative estimates of the displacements produced and of the recovery rate of equilibrium conditions.

Behavioral investigations on stress and perception, performance, and personality variables were briefly reviewed by Pronko & Leith (88). As a relatively recent import from engineering and physicochemical biological inquiry, stress—they insist—lacks a fixed usage in the studies of behavior. The authors use it to designate a set of conditions surrounding a behaving organism (a stressful situation).

It appears that the time is not ripe for insisting on a single definition of stress, a definition that would cover all the phenomena relevant to physiological psychology.

Life stress.—Using psychodrama for staging stressful interpersonal situations, Kalis *et al.* (89) examined cardiovascular responses and the behavior of hypertensive and normotensive women. The hypertensive patients showed a significantly greater rise in both systolic and diastolic blood pressure, and a tendency toward a larger increment in the heart rate. They were characterized as less flexible and adaptive in the stress situations, lacking appropriate assertiveness. It was concluded that hypertensive subjects perceive stress more readily and handle it less well and thus are vulnerable to repetitive rises in blood pressure which may lead ultimately to essential hypertension.

In comparison with physical exertion, emotional stresses have received little systematic attention in regard to the etiology of coronary occlusion. Agreeing that preexisting coronary artery disease is the essential background for coronary occlusion, Weiss *et al.* (90) concluded that gradually mounting nervous tension may be significant.

Studies on the natural history of life stresses, carried out at Cornell University, have been extended to rates of renal excretion water, sodium, potassium and creatinine. The details were presented in the new *Journal of Psychosomatic Research*, published in London (91).

Two approaches were followed in recent studies on psychosomatic aspects of gastric and duodenal ulceration: (a) In man, personality characteristics were studied, using patients with ulcer (92) or high and low secretors of pepsinogen (93). (b) In animals, the role of emotional stress has been investigated experimentally (94, 95, 96). Gastric ulcers were produced in rats placed for 30 days into a conflict situation in which approach responses based on hunger and thirst drives were in competition with avoidance responses based on electric shock.

Selected physical stresses.—Exposure to the physical stress of intense

noise may result in definite hearing loss (97). Industrial deafness of the perceptive type differs from auditory fatigue by the permanence of the hearing loss. A selected bibliography of fatigue and stress (98) has been prepared in connection with the planning of experimental studies on the effect of long flights in an airplane of advanced design. Investigations on work decrement in a complex sensorimotor task were surveyed, with emphasis on the counteraction of the impairment of the work output (99) in reference to stimulants, motivational techniques, and task design.

LeBlanc (100) studied the effects of cold on finger mobility, using water for cooling directly the arm, the hand, or the finger. While stiffness of the joint is a factor of importance (and the more joint movement, the more decrement), a decrease in performance involving repeated flexions of a finger was observed even when the synovial fluid was not affected. In this case the decrement was ascribed to muscle impairment.

One of the important stresses of the jet age is the g exposure (and we do not refer to the "breechcloth worn by savages"). During accelerative stress induced experimentally in the human centrifuge (101), profound changes take place in vision, from dimming of peripheral vision, to the loss of peripheral vision (so-called grayout) at a higher level of the stress to a progressive constriction of the visual field, and finally loss of central vision (so-called blackout). The subject is functionally blind but conscious. If the acceleration is continued, a loss of consciousness occurs. It is suggested that blackout is a retinal rather than a cortical phenomenon, with the block occurring in the ganglion cell layer of the retina.

Irradiation.—In Hiroshima and Nagasaki the injuries from blast and from heat (102) represented a direct and dramatic proof of progress in the effectiveness of warfare. Anorexia, nausea, and vomiting, though less dramatic, were the earliest symptoms of injury from heavy doses of ionizing radiation (102, p. 127), and, biologically, these effects are by far the more important ones. While the world may yet be saved from an atomic war, the peaceful uses of the energy of the atomic nucleus demand that careful attention be paid to the biological effects of ionizing radiation.

The reviewer's task in covering research on the behavioral effects has been lightened by Furchtgott's excellent survey (103). Space-travel enthusiasts will be pleased to know that exposure up to 60 hr. to the low-energy, heavy-nuclear component of primary cosmic radiation present at very high altitudes has no untoward effect on functional capacity of Java monkeys and does not produce changes in over-all behavior (104).

Most of the investigators have utilized irradiations with x-rays. Contrary to the general excessive use of rats as experimental animals, the work on the effects of irradiation has relied heavily on primates. Interestingly enough, tremendous doses (2500 r) of head x-radiation of rats failed to impair significantly the retention of a maze habit (105). There are important species differences in the decline of general activity (106), reflecting the

relative magnitudes of injury to the gastrointestinal tract. In cumulative stresses we may come across some surprising findings. In small laboratory animals, mid-lethal doses of x-irradiation significantly depressed food consumption which, in turn, increased high-altitude tolerance (107).

In a delayed-response test (108), no behavioral changes followed administration of sublethal (150 r) doses of x-irradiation in dogs. Animals receiving a midlethal dose (300 r) showed some deterioration in behavior in the first weeks of the post-radiation period, especially slowing down of movements and reduced attention reflected in the increase in the number of stimulus presentations required before the occurrence of a response. In the late post-radiation period, with hematological indications of severe radiation sickness, these changes became more marked. In addition, the speed of problem-solving decreased, as did the number of correct responses.

Following a large, single dose of whole-body x-radiation there was a general decline in free cage activity of monkeys, with a specially marked deficit in the frequency of manipulatory behavior and a decline in instances of initiating aggression (109). In hand and orally manipulated puzzles the animals exhibited decrements of performance restricted to the period between the 9th and 49th days following x-radiation (110). The principal result was a loss in persistence rather than in ability. No deficits in learned performance were found by Riopelle *et al.* (111). Harlow & Moon (112) applied mild, repeated doses of x-radiation (100 r every 35 days) to rhesus monkeys trained on a series of standard laboratory tests. Radiation resulted in a significant decrease in activity and appetite, resulting in weight loss, but caused no impairment of performance on any learning task.

DRUGS

These are the times of boom for the study of centrally acting drugs (113 to 116). Fascinating hypotheses regarding the etiology of mental disorder are being proposed which may lead to a fundamental break-through in the understanding and therapeutic control of mental illness (117). On the institutional side, in 1956 the Psychopharmacology Service Center was established within the Research Grants and Fellowship Branch of the National Institute of Mental Health, with the assignment (and funds) to implement a broad program of basic and clinical research in this field.

Psychopharmacology has been the subject of book-length publications ranging from popular discussions of "confession drugs" (118) to symposia dealing with methods for the assessment of the effectiveness of drugs (119); electrophysiological and metabolic analyses of tranquilizing drug action (120); pharmacology of drugs labelled psychomimetic (mimicking mental illness, phrenotropic) and psychotherapeutic (ataractic) (121); and of meprobamate (122). The clinical considerations (123) are less relevant for the present purposes, but a new treatise on alcohol, the oldest tranquilizer of them all (124), deserves notice. The literature on drugs and behavior is

large and just about all of the space available for this review could be taken up by this topic.

Drugs are of interest to the physiological psychologist principally as tools which can be used for a better understanding of normal mental functioning and of mental dysfunction. Experimental production of abnormal states of mind by physicochemical agents, such as the lysergic acid diethylamide (125), has stimulated vigorous exploration of the biochemical concept of mental illness and provided means for testing counteracting chemicals.

In addition to experimentation, scientific progress depends on a precise, preferably quantitative description of the phenomena studied. The psychologists contribute techniques for the study of behavioral effects of drugs and thus participate in the integration of biochemistry, pharmacology, physiology, psychology, and psychiatry, a process which offers significant hope for major advances in our knowledge of human behavior.

In the recent symposium on methodology (126) the majority of the reports were devoted to animal behavior, spontaneous and conditioned. The pressing of a lever by the rat and the pigeon's pecking at a panel were utilized as the principal instrumental responses. Miller (127) made two points that should be noted: (a) Methodologically, he advocated the use of a diversity of behavioral measures in order to avoid misleading generalizations. (b) He stressed the need for basic research in the development of a science of behavioral pharmacology and warned against the danger of diverting facilities and personnel to the screening of new drugs, important as this task may be.

Brady (128) has reported the effect of amphetamine and reserpine on conditioned fear response to a clicking noise previously paired with a painful shock to the feet. The method (cf. 129) provides means for the assessment of specific drug-behavior relationships in the affective sphere.

Olds *et al.* (130) have described an ingenious method for the exploration of the selective sites of drug action, using as criterion the changes in the rate at which rats depress a lever and thus produce electric stimulation through electrodes implanted deep in the rat brain. The various parts of the "rewarding" system yield different basal response rates and appear to be differentially sensitive to neuropharmacological agents.

Mescaline in medium doses (35 mg./kg.) inhibits the cardiac component of the excitatory conditional reflex while the motor response (withdrawal of a foot, reinforced by a shock) remains relatively intact (131). Under morphine (132) the split between the emotional (visceral) and motor functions is also present, but here the conditional motor response is inhibited while the cardiac component response is not changed. Stephens & Gantt speak of schizokinesis or dissociation. With high doses of mescaline (70 mg./kg.) both aspects, motor and autonomic, of the conditioned response were found to be inhibited. At the same time conditioned stimuli elicited accentuated emotional vocalizations. The dogs reacted to the sound as if it were an elec-

tric shock. On the basis of these observations a neurodynamic hypothesis is proposed according to which in man under mescaline intoxication and in some mental diseases the secondary signals (words and ideas) act like the primary signals of sensations and direct impressions of reality.

A variety of psychological tests have been applied in man to study the effects of sodium amobarbital (decrement of performance) and chlorpromazine (no impairment) (133); of ultran and meprobate (impairment with heavy doses) (134); and of drugs selected as representative of narcotic, hypnotic, tranquilizing and psychomimetic agents (135, 136). In the study by Kornetsky *et al.* (137) the specificity of the effects was noted, with lysergic acid diethylamide causing significant impairment on intellectual and perceptual tasks whereas chlorpromazine and secobarbital affected motor performance.

In investigations carried out with college students on the effects of moderate doses of commonly used drugs, a broad spectrum of behavioral responses was studied but the adjective lists for self-report of mood turned out to be the most important instrument (138). Psychophysiological criteria were used, together with encephalography and physiological parameters, in a study on the effects of atropine (139).

Abramson and his co-workers initiated in 1955 an important series of investigations on lysergic acid diethylamide in nonpsychotic adults. The communications published during 1956 dealt with drug tolerance (140), effect upon handwriting (141), and desirable reactions which could serve as an adjunct to psychotherapy (142). Bercel *et al.* (143, 144) produced widespread temporary psychotic-like alterations in normal adults. As to the mechanism, on the biochemical side the authors point toward enzyme inhibition, on the neurophysiological side to the primary involvement of the temporal lobe and its connection with the diencephalon.

The search continues for physiological criteria of the reactivity of the nervous system, useful in the theory and therapy of mental disease. Shagass *et al.* (145) applied successfully a test involving a determination of the sedation threshold. Shagass and his co-authors (146) regard the sedation threshold as just one way of measuring excitability of the central nervous system in the intact man. It is expected that a battery of objective tests, like the sedation threshold but measuring different aspects of C.N.S. function, would make an important contribution to psychiatric practice and research.

In the context of researches on the chemical criteria and bases of individual differences in psychophysiological functions and personality it is imperative to note Williams' (147) presentation of facts and theory of biochemical individuality. The implications for psychology were not spelled out in his present volume in detail, but the last chapter emphasizes distinctiveness in brain morphology and in brain metabolism as two additional places to look for the origin of differences in psychological characteristics within the normal limits and in susceptibility to mental disease. Specifically,

it is pointed out that in a population in which the diet is very much alike for all, only certain individuals respond to reduced niacin intake by developing pellagra. Furthermore, there are marked individual differences in the severity and in the time of appearance of the psychotic symptoms, sometimes preceding and sometimes following the typical symptoms of dermatitis and diarrhea. A biochemical explanation of these differences is not yet at hand but the enzyme system of the brain clearly represents the crucial component in the process of derangement of brain metabolism.

HORMONES

The intensified study of psychopharmacology, with new means for inducing "model psychoses" on the one hand and the tranquilizing effects of other drugs on the other hand, has added a general impetus to the inquiry concerning the biochemical factors affecting behavior (psychochemistry). Hoagland (148), in his foreword to the proceedings of a conference on neuroendocrinology has pointed out that hormones are endogenously produced drugs, regulating the internal environment of the cells and modifying many aspects of brain function and behavior. Four groups of products of the glands of internal secretion were considered at the conference: steroid hormones; sex hormones; serotonin, epinephrine and their metabolites; and thyroid hormones.

Chronic administration of large amounts of some adrenocortical steroids affects markedly the excitability of the central nervous system, as measured by the electroshock seizure threshold technique, with deoxycorticosterone acetate elevating, 17-hydroxy-corticosterone acetate steadily lowering the threshold (149). The adrenal cortex exerts its regulatory function, restoring normal brain excitability only when the latter has been altered as in hypoxia. Brodie & Shore (150) have proposed the hypothesis that serotonin is the synaptic neurohumoral agent for the central parasympathetic system and norepinephrine the chemical transmitter in the central sympathetic system—a concept which may be important for the interpretation of action of such centrally acting drugs as reserpine, chlorpromazine, lysergic acid diethylamide, or mescaline.

In lower mammals the so-called sex hormones are essential for the initiation and maintenance of biologically adequate patterns of mating. Additional information is needed on the relationship between hormone action and neural functioning. The use of implanted electrodes may be expected to facilitate the investigations into the neuroendocrine mechanisms of mating (151, 152). In man the problem is much complicated by the superimposition of learned behavior patterns.

A new technique permits a direct chemical or electric stimulation, or both, of restricted brain areas in unanesthetized animals (153), with implant shafts permanently inserted in the brain. By hormonal stimulation of certain loci, long lasting drive-states have been induced. Specifically, in male

rats integrated maternal or sexual behavior was elicited from separate (medial and lateral preoptic) areas of the brain during stimulation with testosterone. With new avenues opening for clarifying the role of hormones in eliciting behavior and the organization of neural circuits that mediate or integrate primary drives, the author feels that a neurophysiological definition of drive seems within reach.

When testosterone propionate is injected intravenously (0.5 mg.) into gonadectomized male and female mice, the typical sex difference in aggressiveness found in intact animals is not abolished (154). The most likely explanation is that the testosterone sensitizes differentially the preorganized mechanisms within the central nervous system. However, the possibility that the females do not provide as adequate stimuli, behavioral or olfactory, as do the males, remains to be examined. The authors discarded the alternative that there is an important nongonadal (adrenocortical) source of androgens which causes greater aggressiveness in castrated male mice. It may be noted that the temporary persistence of sexual behavior in male hamsters after castration is not dependent on extragonadal androgens from the adrenal cortex (155).

Intramuscular injection of cortisone acetate was found to enhance general activity in a closed field (156) in the absence of inhibitory training. Under conditions of fear (electrical shocks) there was a definite decline in activity. Cortisone did not reduce the fear, i.e., did not prevent the inhibition of activity produced by shock.

In a sand-digging task the hypophysectomized rats were found to be somewhat slower than the controls (157). More importantly, while they were capable of relatively high levels of physical performance for short periods of time, their energy reserves were smaller (158).

The endocrines are almost inextricably associated in function with the autonomic nervous system, the other major homeostatic regulator, and much of current research on stress is concerned with the role of these two systems. Hormones serve as mediators and, in part, as measures of response to a variety of stresses. A good, working endocrine system is an efficient cushion between emergency situations and the individual (159, p. 686). Adrenocortical reactions to emotional stress, measured in terms of concentration of 17-hydrocorticosteroids in peripheral blood, have been studied in a variety of real-life and experimental situations (160).

In anxious subjects the control levels of plasma hydrocortisone and of urinary hydrocorticoid excretion were found to be 60 per cent and 70 per cent greater, respectively, than in normal subjects (161), but a stress interview failed to increase further the hormone levels when all the experimental days were considered as a whole. However, when the data were regrouped in regard to the intensity (high, medium, low) of anxiety resulting from the stress interview, significant differences were obtained in the mean increases in the hydrocortisone level.

A marked fall in the concentration of circulating eosinophil leucocytes was reported (162) in anxious students engaged in operative dental surgery under conditions of final examinations. The response is mediated through anterior-pituitary and adrenocortical hormones.

WHAT NEXT?

This reviewer has engaged in the game of peeking around the corner in a survey of recent developments in the field of studies on nutrition and behavior (53, pp. 339-41). He asked: What are the important problems? What are the promising methods? Where do we go from here, in general?

Importance can be assessed in terms of two types of criteria. Theoretically, facts and hypotheses derive their importance from opening new horizons and from closing gaps in our understanding of the phenomena under study. In the last analysis, the practical criteria against which all biological research eventually must be evaluated is the contribution to man's health (including mental health) and productivity, even though comfort and pleasure can not be neglected. Thus food additives and pesticides must be appraised, first of all, in terms of potential toxic and carcinogenic effects. However, the effects, if any, on the taste of fruits or vegetables can not be ignored because these effects will affect the consumer acceptance of the product (163).

Agriculture and the food industry will remain interested in the complex problem of flavor. Flavor control is as essential to the standardization of a product as the food composition. Much of the food technology operates on a prescientific, grossly empirical level. Relations between physicochemical properties, including the composition of chemical compounds, and their sensory effects are inadequately understood at present. In fact, one of the fascinating aspects of the chemical senses of gustation and olfaction is the paucity of psychophysiological knowledge concerning them and the continued expectation of living on the brink of a great discovery (11, p. 131).

Controlled investigations carried out in the laboratory clearly indicate that deficits of calories and of essential nutrients, especially some of the vitamins of the B-complex (thiamine), have profound impact on both components of work performance—motivation and work capacity. Clinical experience extends the range of observations on the role of nutrition in behavior.

While the potential usefulness of quantitative, psychometric methods has not been fully exploited in clinical (or even experimental) studies in man, it is the field work concerned with the appraisal of psychological impact of improved nutrition in undernourished and malnourished areas of the world that offers major challenge to behavioral scientists.

In many parts of the world outright nutritional deficiencies are a grim reality or a constant threat. Elsewhere, as in this country, the problems have shifted and the quest is not for the minimum but for optimal nutrition. This is not, by any means, identical with the attainable maximum. In fact, reduction is the current motto, both in reference to the caloric intake and,

perhaps more importantly, to the fat content of the diet. Attention is directed, understandably, to the category of non-infectious, degenerative diseases, with emphasis on coronary heart disease (164).

Weight control, as a matter of balance between caloric intake and energy output, and weight reduction constitute a fruitful field of research for psychologists of varied training. In view of the accumulating evidence of the biological (165) and health significance (166) of dietary fats, the study of food habits and their modification will represent an important issue in studies on behavior and nutrition. But this may be regarded as the province of psychological physiology rather than of physiological psychology.

There is some evidence (167) that retention of dietary calcium is affected by emotional stress. This points to a whole new area of "psycho-dietetics" involving research, both in animals and men, on the effect of emotional states on nutritionally relevant metabolic processes.

Looking into the crystal ball is a hazardous undertaking, even in a limited field of scientific inquiry. It is a stroke of luck that Harlow participated in the symposium on "Recent Progress and Probable Break-Throughs in the Science of Psychology" and climbed sufficiently far out on the limb to provide a more than adequately high pitch for the final paragraph of this review (168). It is a pity, in fact, that the article can not be cited in full. Harlow sees a rosy future as far as financial support is concerned but stresses the need for the development of adequate so-called small-grant programs. Among the fields with a promise, the following are singled out: the study of physiological mechanisms underlying emotional behavior, combining the neurophysiological and neurosurgical techniques of stimulation or destruction of subcortical centers with behavioral testing techniques; psychochemistry; psychopharmacology; psychogenetics; and studies on the effects of hormones on behavior. High hopes are held out for further development of efficient, refined behavioral tests but the crown is reserved for the psychological theory of tomorrow.

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COMPARATIVE PSYCHOLOGY¹

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This chapter was prepared following a series of visits to laboratories and field stations where comparative psychology is under very active investigation. What has been observed, taken with this year's publications in the psychological, ethological, and biological journals, and the chapters of previous *Annual Reviews*, has given the chapter its form.

The advances in the past year in the topics usually allocated most space in this chapter, and that most psychologists think of when they think of comparative psychology at all have not been great. Some (1, 2, 3) might have been advances if they had appeared when the experimental work was completed. Another paper (4) provides a discussion of brain weights in more or less association with the report of some experiments on learning; the relation established is one of simple contiguity in the pages of a paper. Little new appears on hoarding; investigations of bird navigation seem to have reached an impasse, where the only theory that seems adequate to the facts is untenable (5). [For a popularized summary, see Carthy (6).] The quasi-monopoly of the experimental study of sex behavior in the male *boojum*, or rat, held by Beach and his colleagues, has been broken with the appearance of an excellent monograph by Larsson (7).

The general picture is a familiar one: advances have occurred, but at glacial pace; work on familiar variables inches along. A different picture arises, however, if the work of ethologists and zoologists in the field of behavior is examined in its own context rather than according to psychologists' ideas of what should be important. Here the advances in the last year—in the last few years—have been rapid, remarkable, and of direct relevance to psychologists, whether comparative, experimental, social, or clinical. Ethologists, mostly European zoologists, study the behavior of a number of species from an objective (in the Watsonian or Pavlovian sense) point of view that does not exclude an active interest in physiological correlates of behavior. Previous chapters in this *Annual Review* will have familiarized the reader with some of their work, but they have placed, I think, undue stress on a few sets of investigations and misleadingly emphasized some now obsolete parts

¹ The survey of the literature pertaining to this review was completed in May, 1957.

² The writer wishes to express his appreciation to Drs. G. Baerends, J. van Iersel, W. H. Thorpe, R. A. Hinde, N. Tinbergen, and Konrad Lorenz, and to their colleagues, whose hospitality made it possible for him to observe what is going on in their laboratories. He also wishes to acknowledge his thanks to the institutions whose libraries and other facilities were made available to him, and to those who have reviewed the earlier drafts of the chapter.

of ethological theorizing. What follows is based not only on the current publications of ethologists, but also on visits to their laboratories, and on many and long discussions with them.

Experimental facilities.—Through the past few years, several major research and teaching laboratories have been (or are now being) constructed, whose major features have been determined by the needs of research on animal behavior. The degree of environmental control made possible in these laboratories extends considerably beyond the minimal air-conditioning all too rare in the United States; the very special environmental requirements for the maintenance of, and experimentation upon, a variety of species can be met.

Experimental animals.—Ethologists are more and more settling on a few laboratory species for their experimental investigations. This parallels the development of American animal psychology, and it has taken place for many of the reasons that historically determined the ascendancy of the rat. Analytic experimental research is becoming progressively limited to the stickleback, the canary, the black-headed gull, the bee, the hen, and drosophila. Mammalian studies do not yet show such convergence—perhaps because of the example we have given by our preoccupation with the rat, with its consequences.

Experimental design and treatment of the data.—Over the past few years, ethological research has become progressively analytic, with fuller and more careful control of the variables that must be taken into account in experimental design. The controls are both experimental ones (environmental manipulations) and statistical ones (appropriate designs). Together with these improvements in experimental technique has come a new emphasis on quantification, both in the measurement of behavior and in the association of probability levels with experimental findings. Methodologically, the ethologists have advanced significantly toward the experimental ideal of rigor, for lack of which they have frequently been criticized.

Theoretical orientation.—As one outcome of a number of papers (8, 9, 10) of a series of discussions at international meetings of various sorts³ and of any number of informal talks with experimental psychologists, as well as with each other, most ethologists have radically changed their viewpoint on the interrelationships between genetic structure and behavior: instinct is once again dead.⁴ Unlearned, or innate, means now to ethologists pretty much the

³ One conference, now fully reported, marks a watershed in the "innate-learned" controversy [see (11, 12, 13) and their discussions]. The report (14) includes a number of authoritative summaries of major research areas.

⁴ The status of this concept among ethologists is not well represented in the Thorpe text (15). Neither the theoretical point of view to which most of the book is addressed nor the interpretations given to much of the experimental work summarized is well received by many ethologists, who dissociate themselves, often with asperity, from its propositions. The bibliography is a useful one. See also Beach's review (16).

same as it means to psychologists: what the animal brings with him to a set of observations that he doesn't seem to have had a chance to learn. That is to say, it merely sets a problem. Lehrman's (17) magnificent experimental exposition of how a complex species-specific behavior can be acquired has had its effect, and it is now commonplace to hear referred to as "probably learned" behaviors that a few years ago were categorized without qualification as instinctive or innate. Species-specific behavior is no longer solely attributed to genetic structure when the individuals of the species share prenatal environments and early experience, when they learn quickly and according to the same laws, when they show a common set of physiological mechanisms, and when they spend their lives in remarkably uniform microenvironments. Unfortunately, psychologists do not seem to have learned as much from the ethologists as they from us, for we do not always show the respect for species membership as a determinant of behavior (through whatever mechanism) that we might. [It is not, for example, appropriate to check statements about wild fowl, using domestic hens (18).] We do not match the growing sophistication of ethologists towards the many ways in which environmental events and physiological functions may determine both the structure and the behavior of the individual after fertilization.

The reader might conclude that, except for its superior technical facilities, ethology is becoming indistinguishable from comparative psychology as it is known in the United States. This is true only in part; there remain some characteristics of ethological research that distinguish it sharply from most of what we do. The problems to which ethologists address themselves identify ethology as comparative psychology in the sense that comparative anatomy and comparative embryology are comparative sciences. It relates behavior not only to its immediate antecedent variables but to its broader biological context—to genetics, to taxonomy, to evolutionary theory, and to ecology. This comparative psychology, emphasizing the relation of behavior to general biology rather than solely to physiology, returns to that familiar in the United States during the first decades of this century (but gone with the *Journal of Animal Behavior*). The present definition of comparative psychology in the United States seems to be: all psychology dealing with animals that is not treated under learning, motivation, and physiological psychology; or, to state it another way, studies on animals that do not conveniently fit anywhere else. Again, ethologists are far readier to adopt working hypotheses and to drop them, once disconfirmed, as well. Finally, they are often concerned with certain specific behavioral phenomena (e.g., sign stimuli, displacement activities) that may no longer have any very special theoretical significance but that still present live experimental and interpretative problems.

The research of ethologists, then, retains certain characteristics that differentiate it sharply from that of American psychologists. The biological emphasis has determined in large measure the selection of species and of problems for study. In this context, largely unfamiliar to psychologists,

some of the most interesting recent ethological research has been done. [For a general source on the biological context, see Carter (19).]

Several (necessarily overlapping) major areas of research may be distinguished. The first relates to general surveys of the behavior of one or of a number of species, usually descriptive. These are made under field or zoo conditions and range from superficial surveys to intensive, protracted quantitative observation of large numbers of animals under controlled conditions. Such studies are the indispensable antecedents to analytic experimentation. More than once, in their absence, a psychologist has controlled literally out of existence the necessary environmental conditions for, or necessary behavioral correlates of, behavior that he wished to experiment upon.⁵

The second area is the genetics of behavior, in a sense more restrictive than that in which strain differences in the loveableness of puppies or in the pugnacity of domesticated mice are referred to under the category "psychogenetics." Geneticists study *drosophila*; their genetic structure, and its relationship to morphology, are well known. What of their behavior?

The third area stems from the fact that related species behave alike. Taxonomy today is a science that deals not solely with the classification of species but with the most general problems of their origin and divergence in evolution. Behavior enters into problems related to species in at least three ways; one of them is this: behavioral differences between two populations that are morphologically identical may be the only way of telling them apart. The second relates to tracing out the evolutionary history of a species or of a set of related ones. Finally, behavior is important in maintaining the reproductive isolation that serves to produce and maintain species. Much interbreeding that could occur does not do so only because the behavior (and associated morphology) of an individual of one group does not effectively control the sexual behavior of a member of another. Behavioral mechanisms contribute substantially to the development of new species and evolve with them by fostering genetic isolation.

⁵ Two examples come to mind. Riess (20) showed, in a carefully controlled experiment, that female rats that had never had the opportunity to pick objects up or to carry them about, prior to parturition, failed to build nests and to retrieve their young. He was careful, of course, to conduct the final critical tests in special living quarters provided with nesting materials; the animals were introduced into this just before parturition. Eibl-Eibesfeld repeated the experiment but, being more familiar with the behavior of rats, introduced the females into the experimental boxes several days before parturition, allowing them time to settle down to sleeping in one particular area of the cage. He introduced the potential housing materials at the same time as had Riess. These rats built nests and retrieved, although not as well as control animals. Again, Berry, working with the writer (21), found that rats eat small pieces of food at the place where it is found and transport large pieces to another place before eating it. This finding has implications for experiments on the effect of magnitude of reinforcement on habit strength as measured by running speed and resistance to extinction.

The fourth major area relates the structure and behavior of members of a species to general evolutionary concepts: to adaptation to particular environments and to survival value—that is, to reproductive rates and to death rates. The generalization towards whose verification these studies are directed might be stated as follows: The behavior of an animal, like its structure, is the outcome of the interactions of the animal, and of its forebears, with its environment, animate and inanimate . . . with its predators and prey, its mates and offspring. Pointedly, competition is omitted from this somewhat platitudinous statement of a not always obvious generalization.

A fifth research area cuts across the work of ethologists orthogonally to the above. One of the major contributions of ethologists is their detailed empirical account of the organization of particular individual responses into major response sets, identified by statistical covariance, and perhaps by common sets of antecedent variables. Besides such sets as attack, escape, and sexual behavior, the ethologists have identified a set called threat that occurs when tendencies to attack and to escape are approximately equal (conflict). Threat includes a number of responses called displacement activities (a term that most ethologists qualify with an "in quotes"). The courtship behavior of both birds and fishes, analyzed in detail, includes responses that can readily be related to responses that appear in escape, attack, threat, and copulation proper. Aggressive behavior and fear, that is to say, are closely associated with sexual behavior; studies of sexual behavior, if pursued, lead to studies of aggressive behavior, and sometimes vice versa, unless the experimenter overcontrols the environment.

Displacement activities are those responses first encountered in a context in which their antecedents and consequents have become well understood but that also appear in other, quite different, situations where, in terms of the variables they had been related to, they make no sense. A threatening gull may show behavior appropriate for nest-building; a bird in an approach-avoidance conflict (as food-shock) preens. The conditions required for the production of a given response or series of them as displacement activities, remain a live experimental as well as a theoretical problem of special interest because many of them, ritualized, appear in the courtship behaviors that maintain genetic isolation.

A sixth line of endeavor looks toward the physiological bases of the relationships found—that is, it is concerned with providing a sound theoretical account of the immediate antecedents of behavior. Tinbergen's theory of instinctive hierarchies, expounded in his well-known treatise (22), is obsolete and largely abandoned (especially by Tinbergen who emphasizes the theory's inadequacies perhaps more than do others). Alternatives are being sought.

Finally, something that might be termed applied ethology is developing. To be sure, a very large amount of research on behavior has been sponsored for good and sufficient economic reasons by schools of animal husbandry and the like, but, latterly, a new field of application has developed. This is the use of behavioral assays in pharmacology. A scattering of papers concerning be-

haviors that ethologists have worked on intensively has appeared, the work done by specialists in other fields. The inadequacy of these researches, as well as the striking behavioral phenomena observed, suggest that ethologists will find drug-studies an extremely fertile research tool—and vice-versa.

In the following sections we will present summaries of the past year's work in terms of these areas, recognizing that any classification forces one occasionally to be arbitrary in placing one or another study in this or that category.

GENERAL STUDIES

Hediger (23) reports a large number of observations on behavior in the field, in zoos, and in circuses. His tentative generalizations are a rich source for problems, especially on social behavior, that may be attacked directly in the laboratory. His book should be a valuable supplementary text. Further field observations are reported on whales and porpoises (24), three studies report briefly on the reproductive behavior of fish (25, 26) and scorpions (27), and two (28, 29) provide thorough and detailed descriptions of the behaviors of birds.

Two papers point up the usefulness of general surveys of the behavior of particular species. Dart (30) amusingly emphasizes that a reasonable set of data on the feeding habits of the hyena would settle a long-standing controversy on the origin of fossil deposits in caverns and hence provide a substantial contribution to our data on primitive man. [It is by no means nonsense to refer to fossil behavior when its consequences are available for study.] The other (31), a very careful experimental study, shows that a minority of the domesticated albino rats and a majority of their wild cousins are both more systematic and effective in killing mice than are cats in killing rats. This difference conforms with many others observed and predicted between wild and domesticated strains. A stereotyped attack is usually followed by the eating of part or all of the victim, brain first. Nonkillers could not be induced to kill a mouse even though they ate fresh-killed ones presented when they were food-deprived. Nor could aversive stimulation that produced vigorous intraspecific fights induce these animals to attack mice. Nonkilling females, given live mice at parturition, adopted them, repeatedly retrieved them when they strayed from the nest, and added them to the litters, to the latter's disadvantage since the mice almost always moved into the warmest part of the nest and occasionally ate the new littermates. The survival rate of the litters into which the mothers adopted mice was low. The behavior of these rats towards their mice was very clearly correlated with their endocrinological state. Amygdalectomy significantly reduced the incidence of killing by the killers. This behavior remains unrelated to others of the rat's behaviors that are known, and, in terms of them, is inexplicable; the fault perhaps lies in the absence of a sufficient variety of data on the animal's life history and behavior. The evidence suggests that the tendency of wild rats to kill mice is associated with the increased survival rate of the

litters of mouse-killers in an environment where there are plenty of mice, with *their* tendency to eat neonate rats. This is a type of selection-pressure with respect to which albino rats have not been bred for many generations.

GENETICS OF BEHAVIOR

Strain differences have been investigated with respect to hoarding in rats (32) and audiogenic seizures in mice (33). Hinde (34) has studied the behavior of interspecies hybrid finches (*Carduelines*). If the parent species have identical behavior patterns, then these appear in unchanged form in the hybrids. Other responses, differing in the parent species, appear in intermediate form in the offspring, either quantitatively intermediate (where one species frequently shows the behavior and the other but rarely) or qualitatively (when the topographic patterns of the response vary). Behaviors that appear in only one of the parent species appear either unmodified in the hybrid or they do not appear at all. One significant result should be noted—"the relations between display components and behaviour tendencies are preserved (qualitatively) unchanged in the hybrids." That is, responses that are associated with aggressive behavior in the hybrid are also associated with aggressive behavior in the parents, and similarly for sexual and fear responses.

Of importance for the description of the action of the genes in the determination of behavior is the use of *Drosophila* as experimental animals. Bastock & Manning (35) and Bastock (36) now report on the yellow mutant of *Drosophila melanogaster*, derived from a single gene (sex-linked, recessive). Associated with the gene is a change in both the duration and frequency of the wing-display in the courtship of the male. This altered pattern of courtship is associated with reduced fertilization of normal females by the male, although the courted female behaves toward the displaying mutant as she does toward a normal male. Two other papers also deal with the behavior of *Drosophila* (37, 38); taken together these three papers suggest the fruitfulness of studying hereditary mechanisms in that species whose genetic structure is best known and most easily manipulable experimentally and whose behavior repertory is very small. It is still a bit difficult to see the scientific significance of genetic studies on some other species (39).

BEHAVIOR IN SYSTEMATICS

Taxonomic use of behavior.—Stokes (40) describes two morphologically identical species of gall-midges that parasitize different host-plants within the same genus; they have been classified separately because they produce quite different galls. These species successfully breed only on a plant of the appropriate species. It is not yet known how this happens.

Simmons (41) treats with the head-scratching response of birds, of which there are two topographically differing forms, the same in all genera within a family but differing between families of the same order. Hinde (42), surveying the attack-flight-courtship-and-sex complex of behaviors as well as

feeding, classifies the finches into two major groups, and Andrew (43) distinguishes among four separate classes of buntings. Elsewhere Andrew (44) treats in detail the intention-movements of flight that appear in conflict situations; they act to maintain social groupings. Moynihan (45) presents preliminary data on the hostile aerial behaviors of a number of species of gull.

Mechanisms fostering genetic isolation (segregation of breeding).—For successful mating and for the segregation of breeding necessary for the evolution of a species, individual members of a given member of a strain or species must discriminate between co-members of their strain or species and members of other strains or species. They may or must also recognize individuals. Investigations of the variables and processes governing such recognition are of interest for reasons beyond the evolutionary problems; they also provide data on learning under rather special constraints. Schloeth (46) describes the behavior shown when individuals first encounter other individuals of the same or of different species. This survey covers 814 different encounters. Hale's results (47) demonstrate that hens discriminate the breed of other hens and respond to them on the basis of their group-membership: behavior first displayed to one individual of another breed is generalized to other members of the same breed. In small mixed-breed flocks, one breed completely dominated the other. In larger flocks (about fifteen members), the effect was less complete. Alterations of appearance produced by dyeing or removing the combs of individuals altered the ability of other animals to recognize the individual but members of the other breed were still able to discriminate it from members of their own breed; they showed the appropriate dominant or submissive behavior to it. That is, disguised individuals moved within the social hierarchy with respect to members of their own breed but not with respect to members of the other. With trout, things are otherwise. Newman (48) shows that in general social behavior (dominance-submission relations and territorialism) brook trout and rainbow trout form single interspecific hierarchies in which brook trout tend to dominate the rainbows. One would welcome behavioral studies on the courtship and mating of these two species which must serve to maintain the reproductive isolation necessary for their maintenance.

A special problem of species recognition is that of imprinting. Some species of animals do not come equipped with a battery of species-specific behaviors serving to limit their social behaviors to members of the same species, but with a slight tendency to respond (by following) to a broad class of environmental events (moving objects of a given size range) and a very strong tendency to learn to follow at high strength only those objects which they have had experience in following. In the laboratory, individuals of these species learn to follow boxes, models, and people. One symposium (49) and a number of papers (50 to 54) now permit a set of generalizations whose significance is limited by the fact that not all investigators have worked on the same species. First, imprinting, the greatly increased tendency to approach and to follow a given object, only rarely occurs in the

absence of a history of extensive practice in following. Second, following is self-reinforcing; that is, no reinforcing stimulus has yet proven identifiable. It diminishes in strength with massed practice and with lack of practice; it is not irreversible. Third, practice in following must occur in the first hours and days of the individual's life. Fourth, the strengthened tendency to follow, produced by following a particular object, generalizes in typical gradient fashion to objects like it. Further experiments on imprinting will probably relate the behavior both to flocking (the running-together of members of a dispersed group of neonates) and to the vocalizations made by both the imprinted animal and by the moving object it learns to follow. Vocalizations may be significant whether they are distress cries or comfort (contentment) cries—the small notes given by aggregations of ducklings and goslings when they are together and when they are feeding or have just been fed. The latter vocalizations are analogous to the purring of cats and may be a form of behavior alternate to following in its correlation, once conditioned, with the whole body of social behavior.

Weidmann (54) points out that one probably significant difference between the behaviors of imprinted and nonimprinted ducklings is that when the latter are alone they stand or sit content, whereas the bird that has learned to follow some object gives distress calls, moves about, and in general shows appetitive behavior that ceases when the imprinted object comes into close proximity.

Marler (55) presents a general survey of the responses of birds that are sign stimuli for other birds of the same species. Such releasers tend to be ritualized, that is, highly stereotyped in form. Dealing with the same problem, Morris (56) discusses the probable physiological basis of the feather postures and stereotyped somatic responses that are releasers for one or another behavior of other birds. He shows elsewhere (57) how a stereotyped magnitude of response may develop, both phylogenetically and ontogenetically—that is, through both race and individual learning—so that magnitude loses its usual quantitative relationship to other measures of response-strength such as frequency or rate. The topographic and quantitative stereotype of the releasing response is reinforced by its effectiveness in controlling the behavior of other individuals as a sign or discriminative stimulus. Buchholz (58) illustrates the operation of releasers in maintaining reproductive isolation in dragonflies.

EVOLUTIONARY STUDIES

Survival value.—The three-spined stickleback, *gasterosteus*, stands motionless and erects its spines when it is strongly stimulated by mechanical means. The ten-spined stickleback, *pygosteus*, with much smaller spines, responds similarly to the same stimuli. Fish of other species of comparable size, but lacking spines, do not stand motionless. Pike and perch eat small fish. Three investigators (59) provide quantitative data on the biological function of both the behavior and the spines of the stickleback and avoidance condition-

ing of pike and perch. With successive trials, the predators progressively avoid seizing the spiny fish when more comfortable prey is available. Just how effective this behavior is and how more effective three large spines are than ten small ones is given by the day-to-day changes in the population of a single tank: initially—2 pike, 12 *gasterosteus*, 12 *pygosteus*, and 12 minnows; on day (1)—2, 11, 11, 9; on day (5)—2, 11, 11, 0; on day (7)—2, 10, 2, 0; on day (12)—2, 2, 0, 0; and on day (14)—2 pike, 0, 0, 0. Despined control *pygosteus* proved as acceptable as spineless fish of other species. Predators, on attenuated diets, learned to eat them—but stopped when alternative prey became available. A number of characteristics of the behavior of *gasterosteus* may be related to its relative immunity to attack: it is bold, the males nest on open bottoms, the females school and wander freely, and both sexes show conspicuous nuptial coloration. By comparison, *pygosteus* is a timid, furtive, weed-frequenting.

The biological function of a quite different kind of behavior is shown by Kerruish (60)—in this case by measures of conception rates rather than of death rates, with domesticated bulls as subjects. A period of sexual stimulation (including the presentation of teaser-bulls) and of sexual display and foreplay produced shorter latencies of response to cows as well as enhanced conception rates when the semen collected was used in artificial insemination.

Adaptation of behavior to an environment.—Cullen (61) maintained the ledge-dwelling kittiwake under close observation through three complete reproductive seasons, making a point-by-point comparative study of its behavior with respect to that of the black-headed gull. She was able to enumerate a series of differences between the two species in general behavior and in detailed responses, both releasers and not. Each difference can be related to the difference in habitat. Morphological differences and behavioral differences went together; e.g., the young kittiwake showed neither protective coloration nor cryptic behavior.

The behavior of the bee, and its evolution.—[For a recent summary, see (62).] There is increasing stress on studies that seek to establish the evolutionary antecedents of the performance of this insect in navigating by the sun, in telling time (physiological clocks), and in communicating the location of feeding grounds to its fellows, as well as in the less spectacular features of its social life. Before summarizing these, it is pertinent to bring the reader up to date on some other features of the behavior of bees. First, swarming is coming under investigation (63): prior to swarming, the bees display a greatly increased activity level and their behavior with respect to the old queen changes; the number of attendant workers falls off and they often fail to feed her. At the same time, virgin queens commence to pipe, progressively increasing in frequency up to the time swarming occurs; this piping produces a cessation of activity in nearby bees. [This observation may be related to the Frings & Little report (64) that pure tones between 300 and 1000 c.p.s., at 110 to 120 db. sound pressure at the source, produce a complete cessation

of activity inside a nearby hive; at swarming time a lowering of threshold for such quiescence may occur.] Communication plays a very important role in determining the behavior of swarming bees (65). A number of scout bees go out from the swarm; after each encounters a possible nesting place she returns to the swarm and performs a dance whose vigor is related to the appropriateness of the nesting place: small, sheltered places produce excited and persistent dancing, with frequent starts of flight in the direction of the site; poorly adapted places (less sheltered and too large) produce less enthusiastic dancing. As more scouts return from various possible nesting sites, the competition between them has much the effect of political orations: the more, and the more excited, scouts that start off in a given direction the more probable it will be that the whole swarm will follow after a number of preliminary starts and even of incipient fissions. The analogy with a New England town meeting is a tempting one.

Two further bits of information on communication must be included (62). First, the language of bees varies from strain to strain and there is even a suggestion of local dialects within a strain. Second, for some time before newly hatched workers go out on their first collecting trip, they follow the dances of returning foragers within the hive. Through this training period, they follow the dance of the communicating bee with increasing precision. Exchanges of food between bees form an important part of the economy of a hive; Free (66) finds that the stimuli controlling both the begging and offering of food are received by the antennae of the beggar when it comes in contact with the lower part of the head of another bee (or head-model). Neither head-color nor shape is important in producing the begging behavior. Williams & Williams (67) present a mathematical model describing how selective breeding for a behavior such as food-offering may occur. This is an example of a mathematical evolutionary theory of a sort perhaps unfamiliar to psychologists.

Manning (68, 69, 70) has been investigating the behavior of bumblebees in gathering nectar. These experimental studies, employing models (baited and scented), provide evidence on the joint evolution of the bees and of the flowers from which they collect food and which they pollinate. These papers provide interesting sets of data showing that the foraging procedure of the bumblebee varies with the type of flower from which it is collecting, and that this behavior is mediated in part by learning processes.

Kalmus (71) presents an evolutionary problem: in the southern hemisphere the sun moves counterclockwise across the sky. Honey bees, established in the hemisphere since European colonization, maintain excellent orientation with respect to the sun throughout the day. The offspring of queens recently imported from the northern hemisphere showed a large angular disorientation in the morning with respect to a feeding station where they had been fed the previous evening, and improved progressively through the day until they were correct in the evening. Hybrids also showed such erroneous orientation.

Shepherd (72) addresses himself to the internal clock mechanisms that have been postulated to account for the foraging, navigational, and feeding behaviors of bees, and reports on a simple activity cycle in the distantly-related fruit-fly that shows the same properties with respect to time as does the temporally-controlled feeding behavior of the honey bee. The bee's temporal behavior may have evolved from such a physiological rhythm.

Dethier (73), also working with flies, demonstrates behavior possibly antecedent to the communication dances. Flies walk circuitously after ingesting food, and their walking is a function of many of the variables that govern the communication dances of bees. Diethier argues, from the "striking parallelisms between the gyrations of the fly and the communication dances of the bee," that common mechanisms are operating and that the bee's behavior in communicating can best be accounted for in terms of a stimulus-response analysis based on the simple behaviors exhibited by the fly and on the physiological mechanisms governing them. He notes that the basis of trophallaxis may lie in his observation that flies frequently regurgitate food and then clean their appendages when their crop is full. Unfed flies in the vicinity become greatly excited, follow and move around the sated individual, and attempt to lick food from its mouth parts.

ORGANIZATION OF BEHAVIOR, DISPLACEMENT ACTIVITIES

Aggressive behavior.—Clarke (74) reports on the responses involved in threats and fights between voles. Godfrey (75), working on four races of the same species and of cross-breeds between them, finds that the offspring of matings between a female of an unaggressive less territorial race and a male of another race, although more hardy before weaning, showed higher mortalities afterwards as well as a failure to show the submissive postures strongly developed in the more aggressive paternal race. The evidence suggests that the higher death rate is attributable to the inability of the loser of a fight to stop the attack of the winner before it is killed, and (since the same effect does not occur with the genetically comparable hybrids of the opposite cross) that submissive postures are learned in the course of association with the mother. These voles show a marked preference (under olfactory control) for members of their own race as sexual partners.

In groups of chaffinches (76 to 79), a straight-line,⁶ peck-right hierarchy is set up as a consequence of repeated fights among members of the group. In it, males dominate females. The more dominant birds are less likely to attack animals markedly inferior to themselves in the hierarchy.

In another experiment, in which the distance between adjacent food-boxes was systematically altered, Marler was able to show that the number

⁶ A very preliminary report (80) shows that such a simple social hierarchy is set up in cats under conditions that are quite restricted relative to the conclusions drawn. The more complex hierarchies, set up in freely moving groups, are illustrated by Chance (81).

of fights was inversely proportional to the distance between birds—a verification of Hediger's concept of individual distance. The distance-fight function has the form of a psychophysical threshold one. Hunger did not affect directly the tendency of the birds to fight, but it reduced the avoidance and fear behavior of socially inferior birds, thus bringing them into closer proximity with a superior one and hence inducing fights. Fighting proved independent of food-getting behavior (although it may be reinforced by food) and of hunger. Marler was able to improve the position of a female with respect to other females in the hierarchy by dyeing her underparts red, simulating the male. Such disguised females won more fights with other females, but they showed fear and escape behavior to the red-breasted males and thus induced attacks. Only if such birds had been hand-reared and had no experience in being defeated by males, did they prove able to fool the usually more aggressive males and hence win a significant number of fights with them.

In a final experiment, Marler found that aggressive behavior—fighting—may be reinforced by, for example, food-getting, so that a bird that has frequently fed quickly and well following a fight that drives off another bird eventually comes to look for a fight and, if confronted with two food-dishes at one of which a subordinate is feeding, will choose the same dish, driving the subordinate off. Marler finds no evidence for fighting as other than an instrumental response indulged in only with respect to other activities, e.g., food-seeking and sex. He states: "It may be that aggression is never caused by frustration, except in a few higher animals, with more complex learning or perceptual capacities."

Sex and aggressive behavior.—Wood-Gush and his colleagues (82 to 85) and Fisher & Hale (86) have been concerned with the aggressive and sexual behavior of the male domestic fowl, *gallus domesticus*. The former find no relationship between mating-rank (frequency) and semen-quality, but a positive one between mating-rank and fertility, quantity evidently compensating for quality. A highly significant correlation between an index of comb size and mating frequency suggests an endocrinological basis for the significant individual differences found in copulatory activity. In other studies no correlation was found between aggressiveness or place in the social hierarchy and mating frequency. Cocks reared in isolation and then introduced to females for the first time and who respond to them aggressively require far longer to attempt to mate or to mate successfully with the female. The less aggressive individuals showed sexual behavior more readily. Fisher & Hale, studying the releasers for aggressive behavior and sexual behavior, found that the posture of a second bird is the most important stimulus determining which will occur.

Among the mammals, Schwartz (87) reports that male rats can be conditioned to bar-press with opportunity to copulate as reinforcing stimulus. Hayward (88) reports that avoidance conditioning of young males with respect to oestral females reduces both precopulatory behavior and the num-

ber of intromissions but does not eliminate sexual behavior altogether. Larsson (7) investigated the recovery of sexual responsiveness following copulation and ejaculation in the male rat; his procedure included a technique of pitting sexual behavior against a food-reinforced barpress. He finds two mechanisms necessary to account for his results; he distinguishes between the recovery functions of copulatory and ejaculatory reflexes. It is to be hoped that this work will receive the attention that it deserves. Beach & Jordan (89) also postulate two mechanisms, an arousal mechanism and a copulatory mechanism, to account for their data on much the same problem.

Closely associated with the aggressive-sex complex of behaviors, as we have seen in Marler's work on chaffinches, is the problem of territorialism. This topic, foreign though it is to psychologists (although they are daily confronted with their children refusing to share their toys and with the universal phenomenon of private property), is summarized in a series of papers in *Ibis* (90), as well as discussed by Hediger (91).

Displacement activities.—Two papers by Andrew (92, 93) reflect current ethological thought on these behaviors, which are now rarely interpreted as sparkings-over. Displacement activities occur in conflict and frustration situations; they are often body-care behaviors. Andrew's point is a simple one: the animal, aroused to some activity by one set of stimuli and prevented from completing it by another set or by the absence of critical stimuli in a sequence of them, may be expected to respond to the one set of stimuli always present. When competing tendencies are balanced, these weak stimuli originating from the body surfaces can be effective—so the animal preens, face-washes, or the like. Andrew extends the argument to a variety of similar responses. His straightforward account suggests experimental tests of this view of displacement activities, and again shows how far removed ethologists now are from the theories expressed some years ago (22).

PHYSIOLOGICAL BASIS OF BEHAVIOR

General.—Two papers are suggestive with respect to the development of a physiological theory of behavior. Weidmann (94) has made observations on gulls that have profound implications for the roles that hormones may be assumed to play in determining behavior. If one places three eggs in the nest of a pair of black-headed gulls several days before the gull lays, it "is possible to suppress egg-laying completely or partially." That is, if wooden eggs are placed in the nest at a time when they are an adequate stimulus for the production of brooding, then the brooding behavior in some as yet undetermined way inhibits the growth of the maturing follicles, and in fact causes them to degenerate, provided they have not yet reached a critical stage of maturity. A second and equally interesting implication of this observation comes out of the behavior of the male, whose brooding behavior parallels that of the female. The effects that are produced through hormonal action in the female are produced in the male by the female's behavior.

The second paper is a lengthy theoretical survey by Prechtl (95), and again shows the shift in ethological thinking in the past few years. This thorough survey of the physiological literature deals with the problem of isolating known physiological mechanisms that have the properties that must be postulated on the basis of behavioral studies of stereotyped movements; it marks a clear departure from instinct theory and makes theoretical suggestions appropriate for learned behavior as well. Models embodying the principles of servo-mechanisms, together with well-established physiological processes, may now be developed.

New directions.—The effects of various drugs on behaviors such as the ethologists have experimentally analyzed with good effect have come under investigation in the hands of others than behaviorists. The results strongly suggest that ethologists may find drugs a valuable research tool and that they can make a very important contribution in this field. Two investigators (96) found that they could make guppies swim backwards. A more ambitious study of the Siamese fighting fish (97) used the fighting response as an index of the effectiveness of a number of drugs including reserpine and morphine. In general, the antihistaminics knocked out fighting behavior; morphine made the fish very aggressive. Enough is known of the behavior of the fighting fish, and of other fish, to suggest perhaps half a dozen mechanisms, alterations in whose function might produce such effects. A sophisticated observer could distinguish among them by simple observations. The following quotation about the effect of one set of drugs, read to ethologists, produces smiles: "another peculiarity was that they became pale upon exposure to a control fish exhibiting the fighting response." This is not peculiar—it is the standard response of a subordinate fish to a superior in a hierarchy, and it inhibits attack by the latter. Ethological studies of drugs may be expected to make a substantial contribution, not only to pharmacology, but also to the data indispensable for the development of an adequate physiological theory of behavior (98).

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DEVELOPMENTAL PSYCHOLOGY^{1,2}

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INTRODUCTION

Classification and grouping of the studies appearing during the year, in the field of developmental psychology, has obviously been a problem for each of the previous contributors to this chapter in the Annual Review. The present reviewer has not been immune to this difficulty. There are studies whose primary resemblance to one another is on the basis of the area or content within which the variables fall: family relationships, learning, aggression, child-rearing practices. Another attractive category system is the degree of manipulation of experimental conditions, from careful conscious control by the experimenter to the selection of naturalistic situations from which variations in experience or conditions can be deduced and possibly measured. Resemblances also occur in the methods used for measurement, and since the total field of developmental psychology is not yet stronger on methods than on theory (or vice versa), comparison of results from studies using similar methodology is still of much interest and value. Differences and similarities in approach of the researchers based on theoretical considerations appear in a number of the contributions.

Since the effects of early experience on the developing organism appears to be one of the more fruitful and currently active areas of research, this has been used as the first division of the review. Generally speaking, such studies seek to determine antecedent conditions and their consequent effects or correlates in behavior. Next considered are groups of studies concerned with processes of learning and cognition, and analysis of various drive systems prominent in the adaptation of the child to his environment. Self-other relationships, including identification, empathy, and self-image studies are followed by investigations of children's social interaction and behavior. Grouped together at the end are those studies concerned with mental and physical functioning—intellectual and constitutional factors. Briefly cited are some of the textbooks which have appeared during the year in this area.

EFFECTS OF EARLY EXPERIENCE

SEPARATION FROM THE MOTHER

Heinicke, (58), working from the Tavistock Clinic and the Institute of Human Relations, pursues the hypotheses of workers in those places on the

¹ Literature cited covers the period April, 1956, to April, 1957.

² The sections on Demographic Variables, Learning, and Cognitive Processes were written by Robert R. Sears, Department of Psychology, Stanford University, Stanford, California.

effects on the young child of separation from the mother. He made extremely careful observation of and used permissive doll play with two small groups of two-year-olds; one group was living in a residential nursery and one coming daily from home to a day nursery. Results are interpreted as suggesting that absence of the mother, particularly if extensive, upsets the two-year-old's normal balance between his impulses (particularly negative ones) and his power to control these in relation to the outside world. The mother normally helps provide support in control and daily affection which strengthens the positive side of the child's feelings, but with a lengthy absence the child shows both negative feelings and demanding behavior toward other adults as parent-substitutes.

Bowlby *et al.* (17), with considerable ingenuity, located a group of children who had been separated from their parents for an appreciable period of time but for whom the reason for separation presumably did not involve any rejection of the child. Their subjects were 60 children, currently from ages 7 to 13, who had been admitted to a tuberculosis sanitarium before the age of four and had spent from a few months to over two years there, separated from their own families. Each of these children was matched to three of his school classmates who served as controls. The results do not show a striking difference between the experimental and control groups: (a) there was no difference in IQ; however, (b) somewhat fewer experimental than control children could respond well in the test situation, according to the examiner's judgments; (c) according to teachers' reports, more symptoms of maladjustment appeared in the experimental children, of two types: first, withdrawal and apathy; second, roughness and tempers; (d) few of the experimental children were found to be delinquent, and over half of them made friends well. The authors' own statement summing up these findings is worth quoting:

It is concluded that some of the workers who first drew attention to the dangers of maternal deprivation resulting from separation have tended on occasion to overstate their case. In particular, statements implying that children who experience institutionalization and similar forms of severe privation and deprivation in early life *commonly* develop psychopathic or affectionless characters are incorrect.

Note that this experiment employs institutionalization as the factor separating child from parent, but unlike previous experiments with more striking results, keeps out the influence of the often emotionally upsetting family conflicts which may be involved when a child is sent to a child-care institution for reasons other than physical health. Moreover, it is extremely valuable to have a study on this important question carried out with adequate controls.

A study of quite different character, but also involving a kind of "separation" from the mother, is Albino & Thompson's (1) description of the effects of sudden weaning on Zulu children. In this culture, the paternal grandmother ordinarily decides, sometime in advance, on a date for the child's

weaning. Previously the child has had unlimited access to the breast; on this day the breast is smeared with bitter aloes and offered to the child at intervals during the day. The two investigators, who are psychologists at the University of Natal, examined the consequences of this procedure with careful interviews with the mothers and other relatives, and by observations of 16 babies between 15 and 24 months of age. The authors' conclusions about effects of this experience are that the weaning is disruptive and sudden, but also socializing and maturing. The child gives up dependency on the mother and becomes an independent being with perhaps a sudden apparent development of ego.

This study is tantalizing. The situation presented is an excellent one for comparison to, for example, Heinicke's (58) situation, where children of close to the same age were deprived of the physical presence of the mother, family, and home surroundings. It is to be hoped that we will have more studies of the situation from these investigators, and in a form from which comparisons with other situations can more readily be made.

CONTROLLED VARIATIONS IN THE SOCIAL ENVIRONMENT

A remarkable and most important experiment was performed by Rheingold (110). Its purpose was to explore the effect upon infants of an experimental modification of the environment. For a group of institutional babies one "mother" was substituted for many "mothers" and the effect upon social responsiveness was assessed.

Sixteen infants, six months of age, healthy, and having lived in the institution for at least three months, were divided equally into experimental and control groups. Under experimental conditions, one person (the experimenter) cared for, "mothered," fed, bathed, diapered, soothed, held, talked to, and played with the eight babies of the experimental group for seven and one-half hours a day, five days a week, for eight weeks. During these hours no one else cared for these babies, although at times other persons were in the room. The experimenter deliberately and consistently tried to adapt her care of the individual needs of each baby as these were apparent to her.

During the experimental period of eight weeks, the experimenter had no contact of any kind with the control subjects, except for brief participation in tests conducted weekly. The eight control babies had their cribs in another room and were cared for by hospital personnel and volunteers according to the routine of the institution. Time sampling observation of the social environments of the two groups showed wide divergences: the control babies were shown to be alone more, and they were observed to be receiving caretaking acts on 7 per cent of the observations, while for the experimental babies the figure was 23 per cent. These caretaking acts, for the control babies, were performed by approximately 17 different persons, while for the experimental babies it was always the same person.

Tests of both groups of babies were carried out by another person (called

the examiner), a stranger (initially) to all the babies. Results showed that the experimental babies did become more socially responsive to the experimenter than the control subjects did. On Cattell and other tests of development, the experimental subjects made slightly, but not significantly, higher scores. The conclusion from the experiment is that the social behavior of the infant may be modified by environmental events, particularly as these events are mediated by one person rather than many. It is interesting to observe that the responses of all babies, experimental and controls alike, were predominantly positive when approached by an adult. This was a "benevolent" type of institution, and all babies were cared for with kindness. Yet the development of more friendliness and outgoing behavior was associated with the more intensive and individually oriented care by the single person.

A related type of experiment was performed by Melzack & Thompson (89), using as subjects Scottish terrier puppies. Twenty-one puppies formed the experimental group; these were kept, for the first seven to ten months of their lives, in cages having cardboard walls so that nothing in the environment was visible except the ceiling. A "free environment" group of 16 puppies was raised as pets in the laboratory or homes. Both groups were tested for social behavior after the restricted group was released from the cages. Tests of dominance showed that the restricted dogs were very inept in a competitive situation with another dog. On tests of curiosity, the restricted group showed diffuse, unorganized excitement rather than sustained or well-oriented curiosity. They also responded with excitement to the approaches of a "friendly" or "bold" man. Both restricted and free environment groups showed "aggressive stalking" behavior in response to a "timid" man. Evidently the postman does not have a chance if he acts timid, no matter how the dogs he meets have been raised.

Two studies by Levine and others (82, 83) have investigated effects on infant rats of early handling and shock. Since it was desired to investigate truly infantile experience, the rats were given experimental treatment during the first 20 days of life. A number of different treatments were used, involving handling by the experimenter, and shock. At 60 days of age all groups were tested on a conditioned avoidance learning task and extinction procedure. Results showed that the No-Handling group was significantly inferior in its ability to learn the task as compared with the two other groups. These subjects also took longer to make their first avoidance response and required more pushing during the early stages of learning. The Shock-Handling and No-Shock-Handling groups did not differ in their initial behavior in the test situation nor in their performance early in learning. In the over-all measures of learning, the Shock-Handling group was significantly poorer than the No-Shock-Handling group. Shock subjects were also more resistant to extinction. Further, the subjects handled in infancy showed significantly greater emotional stability, as measured by low scores on defecation, freezing, and activity level at the time of adult testing.

EFFECTS OF CHILD-REARING PRACTICES

Two large studies of child rearing have come out during the year. Wittenborn and collaborators (153, 154) investigated the development of children living in adoptive homes. These children had been examined at the Yale Clinic of Child Development before they were 14 months of age, and were currently between four and eight years of age. There were two main questions for which answers were sought: (a) How accurately and in what respects can the Yale Developmental Examination of Infant Behavior be employed to predict future development of adoptive children? (b) What characteristics of adoptive homes (placements) may be shown to be correlated with characteristics of adoptive children?

The first question is essentially one of predictive validity of a standardized and well-known infant test. Results on this will be discussed under a later section on intelligence. The second question involves the relationship between development of the children and characteristics of the home, i.e., the effects of practices and conditions of child rearing experienced by the child over the three or more years that he lived in the home as an adopted child.

The second study, *Patterns of Child Rearing* (119), is one part of a larger research study done at the Laboratory of Human Development at Harvard. Lengthy interviews were conducted with 379 mothers of five-year-olds, to explore aspects of child rearing, mothers' attitudes, and their observations of behavior of the children with the aim of providing (a) information which could be related to certain aspects of each child's personality and (b) information about how a fairly representative sample of American mothers raise their children. This book is concerned chiefly with the second point. It provides normative data on child-rearing practices for two large social class groups, examines differences in child rearing for boys and girls, for children of differing birth order, as well as reporting antecedent-consequent data based on mothers' perceptions of the child's behavior. Part of the larger study, but not reported in this book, is the relation of child-rearing variables to performance of the children in standardized situations. One paper, by Levin and Sears (81), has appeared during the year and will be reported in the section on Aggression.

Since there are strong resemblances between the methods and some of the findings of the *Adoptive Children* study and *Patterns*, they will be discussed together. Both used a long interview for gaining from the mother information on the conditions and practices of child rearing, then scored the interview by somewhat different techniques. For *Patterns*, much of the data is reported in terms of percentages of the group falling at given points on a rating scale; a factor analysis was also performed. The *Adoptive Children* study used a cluster analysis technique.

The dimensions "warmth of mother-child relationship" (*Patterns*) and "rejection" (*Adoptive Children*) are probably based on roughly similar data. *Patterns* found the cold mother to report a child who has feeding problems,

persistent bed-wetting, is highly aggressive, shows emotional upset during severe toilet training, and is slow in conscience development. Coldness did not correlate with dependent behavior reported by the mother. *Adoptive children* also found rejection not correlated with dependence on adults, as reported by the mother, but there was a positive relation between rejection and dependence as reported by the child himself in the social reaction interview. Also, when mother-interview data were used, positive correlations were found between rejection and clusters of scores characterizing the child as compulsive, aggressive, fearful, and anxious (the last for the older sample only). Similar correlations appear with the cluster "unsympathetic parent," as also with the cluster "severe toilet training." Interesting results appeared on toilet training in the *Patterns* study. Bed-wetting was found most frequently in children who had suffered severe toilet training by mothers who were cool and undemonstrative in their affectional relations to their children and who had high sex anxiety themselves. Equally high sex anxiety associated with warmth of affectional relationship and gentle training produced very early and successful night dryness.

Punishment for aggression was found in both studies to be correlated positively with highly aggressive children, as reported in mother interviews. The *Patterns* study found that the combination of punitiveness and permissiveness was related to the most aggression. There was one relationship, in the *Adoptive Children* study, with a cluster from the child interview: "cooperation with authority" correlated negatively with punishment for aggression.

More will be said about both these studies in other connections. The *Adoptive Children* study has excellent material on intellectual development, and has done a magnificent job in teasing out the vexatious factor of selective placement in evaluating the effect of the environmental factors on development. *Patterns* is valuable for its report of child rearing as it is practiced currently in a reasonably good sample of the American culture, and for the use of high level theory in a book which can be read by the intelligent layman. Both studies give much attention to careful methodology, within the limits of what is practicable with masses of data and large numbers of subjects. Both are conservative in their pronouncements. They represent a modern scientific approach to the complicated problems involved in estimating affects of differing conditions of child rearing on the young child.

It is interesting to find another study (64) which bears certain resemblances in methodology to the *Patterns* investigation, but differs in that the mothers were interviewed for child-rearing practices when the children were closer to the infancy period. Hubert & Britton, working at Pennsylvania State University, interviewed 56 mothers who had children of about eight-months' age. The same mothers were interviewed a second time when the children were about 19 months. The data are analyzed to show relationships among practices during the first year, during the second year, and also

the effects on the child in the second year (as described by the mothers) associated with practices of each of the two years. There appear to be some rather pervasive attitudes and practices which come out in different form as the child grows older but retain much of the same quality. This study gives one a certain confidence that the behavior of mothers does show some patterning, probably based on the mothers' own internal organizations and ways of reacting. Yet, this same study finds age, ordinal position, and sex of the child to account for even more variance than the presumed personality organization. Evidently these latter variables will need to be considered in every such study, for apparently they exert a compelling influence on how the mother's own feelings find expression. Hubert & Britton's material on this will be considered with that of other studies in a later section.

The classic study of Baldwin, Kalhorn & Breese (6) has been replicated, this time in Australia. Pentony (105) used almost identical methodology, and repeated the Fels finding that children from democratic homes were more active socially, more constructive in play, and show more intellectual curiosity than children from other types of homes. Democratic parents were also found to encourage more group play at home, to be more interested and to participate more in the play centers, to be more highly educated, and to be of higher socioeconomic status. Since these factors are positively correlated with "democracy," the question arises as to whether democracy in the home is the most operationally precise variable to use in this proven relationship.

From the University of Ceylon comes another study on effects of child rearing. Straus (142) used the Rorschach and a translated form of the California Test of Personality with Sinhalese mothers and their third grade children. The mothers were also interviewed for feeding and toileting routines. All results attempting to relate the personality test material to anal and oral frustration, measured by severity of the feeding and toileting procedures, were negative. Also presented were percentages of children treated in various ways, and these may be compared with figures on American children as given in studies previously mentioned. Compared to percentages given in *Patterns of Child Rearing* (119), these Sinhalese children were fed on schedule or on demand to approximately the same extent as the American sample, but were weaned much later. Bowel training was begun much later also. The negative results relating oral and anal frustration to personality characteristics must then be seen as testing the relationship in a situation in which the frustration (if it is that) comes when the child is considerably older than in the American studies.

EFFECTS OF DEMOGRAPHIC VARIABLES

Another approach to the discovery of causes of individual differences among children is by way of demographic correlates. These variables refer to qualities that divide the population into groups by reference to biological, sociological, or geographic characteristics. The main ones that have been

related to child or parental behavior are family size, ordinal position of children, spacing between siblings, sex, occupation (and other indices of socioeconomic status), and race.

Family size and intelligence.—Early studies of the relation of family size to intelligence showed a slight negative correlation. Children from small families tested higher on standard intelligence tests than children from large families. As medical advances decreased infant mortality, and as free public education eased the economic brakes on fertility, there seemed to some geneticists a possibility that the mean intellectual status of society was doomed to a lowering. Through the first half of the twentieth century, however, the size of families decreased rather than increased, and there is ample evidence of a notable general increase in tested intelligence in both Europe and America. Now, with the recent sharp increase in intended size of families in the United States, the question of the effects of this variable on intelligence has again become of interest.

In an excellent evaluation of the large research literature relating to this problem, Anastasi (2) has reached two significant conclusions. First, through the great middle part of the population, there does appear to be a low but fairly stable negative relationship between intelligence and the number of siblings in a family, perhaps of the order of $-.12$ to $-.25$. The relationship is not linear, however; in the uppermost educational levels, the correlation may actually be positive. Second, there is a slight positive correlation between the intelligence of a pair of parents and the size of the completed family they produce.

Existing data are not sufficient to indicate how much of the over-all improvement in intelligence test performance of the last four decades is a product of smaller family size and how much comes from greater educational experience. Each has evidently contributed some part. Anastasi suggests that the frequency and quality of adult-child contacts are significant influencers of child intelligence, and cites several bits of indirect evidence that the differences in these respects produced by large or small numbers of siblings may be responsible for the obtained correlations.

Ordinal position.—Just as with number of siblings in a family, so the position any child holds in a series of siblings determines in some degree the experience he has. The past year has seen the publication of several papers that deal with both the child's behavior and the child-rearing experiences associated with various ordinal positions. Koch has reported further on her beautifully designed study of the behavioral correlates of ordinal position (74 to 77). She selected 384 five- and six-year-old children from native-born, intact, urban, two-child families in Chicago. The children were grouped according to four criteria: sex, ordinal position (first or second), sex of sibling, and space between the subject and his or her sibling. The first three of these criteria provided dichotomies, while the last yielded a trichotomy (two years or less, two to four years, more than four years). The smallest subgroups thus contained 16 subjects each.

In the four publications which have appeared this year, Koch has reported a large number of characteristics of the children's behavior that appear to be correlated with these variables. The measures so far reported have been secured by teacher ratings, largely on scales taken from the California Behavior Inventory for Nursery School Children and the Fels Research Institute Child Behavior Scales. Among the variables measured—some by several separate scales—were sociability, aggressiveness, competitiveness, reactivity to frustration and defeat, apprehensiveness, activity, work habits, responsibility, and sissiness-tomboyishness. The significance of relationships between the demographic variables and the mean teacher ratings was determined by analysis of variance.

With 24 subgroups, the relationships proved extremely complex, of course, and no summary of the findings is possible within the scope of the present review. Indeed, it would be impossible to make a simple summary, for the main relationships appeared to involve the influence of interactions between the four demographic variables rather than simple single-variable influences. Even sex of child revealed extremely few stable single-variable relationships with the behavior measures. In most instances the spacing between siblings and the sex of sibling appeared to be as major determinants of behavior constellations as were sex and ordinal position of the child himself. A few specific conclusions are worth special note. (a) Ordinal position differences are most strongly influenced by the child's relation to his sibling when the spacing between children is least. The greater the spacing, the greater the relative influence of parent-child relations. (b) The two- to four-year spacing, especially for males, seemed to be clearly the most stressful and stimulating. (c) Having an opposite-sexed sibling is more stimulating and security-taxing than having a same-sexed sibling, especially for first-borns. (d) A boy with a much older sister is distinctly more withdrawn and dependent than a boy with a much older brother. (e) The social expansiveness of males varies positively with the distance between the child and his sibling.

The net outcome of this research project is by no means limited to such a complex molecularization, however. Having 24 patterns of behavior to account for, Koch used a considerable amount of wisdom, together with information secured from as yet unpublished interviews with the child subjects, to suggest hypotheses concerning experimental factors that might have produced the behaviors. For several decades the parent-child relationship has received attention as a major source of motivational variation. Now, by examining five- and six-year olds in 24 groups defined by four demographic variables, Koch has called attention to significant variation that must be referred to sibling relationships. Earlier clinical work (e.g., by David Levy) had not left this matter unconsidered, but the prevailing Freudian and Adlerian emphasis in personality theory have placed siblings more in the role of interferers between parent and child than in the role of direct influencers. By what is admittedly a somewhat nontheoretical

approach, Koch has opened up the possibility of constructing a theory of family constellation that may ultimately modify quite sharply our current notions of parental child rearing as a source of the young child's personality.

Such data on child rearing as those presented in *Patterns* (119), however, are needed to test the hypotheses or validate the assumptions made by Koch. The 379 five-year-olds in that study were approximately equally divided among four ordinal positions; only, oldest, middle and youngest. A comparison of the predominant child-rearing practices associated with each position revealed a number of differences. For example, youngest children received the least breast-feeding and suffered (along with *only* children) the most severe weaning. Oldest children, regardless, of sex, were most commonly disciplined by the father when both parents were present, and the father was more commonly the stricter of the two parents when he was dealing with the oldest child. [Interestingly enough, high school and college students, 1335 from Massachusetts and 617 from Tennessee, reported these same facts about disciplining done by their own parents (59).]

One assumption made by Koch was not substantiated by the *Patterns* data—namely, that oldest children are least permissively treated with respect to aggression and quarreling between siblings. In fact, for the *Patterns* sample as a whole, the oldest children were given most permissiveness in this connection, and in the subsample of three- and four-child families, they were also most permissively handled with respect to fighting with neighborhood children. There were no ordinal-position differences in permissiveness or punishment for aggression toward parents, though in general the oldest children received more punishment, especially physical punishment, and their parents disagreed more frequently about child-rearing policies.

Burke (22) was unable to find personality differences between 25 only and 25 non-only college girls with respect to Rorschach and Minnesota Multiphasic Personality Inventory scores, although he did find a slight tendency for the only girls to be less "activity" and more "study" oriented.

Sex differences.—Phillips (107) found that a larger proportion of boys than of girls was referred to a guidance clinic. This finding was replicated on an even larger clinic sample by Gilbert (47). Of 2500 cases, including over 500 from each of two orthopsychiatric and two psychoeducational clinics, the ratio of boys to girls referred was $2\frac{1}{2}$ to 1. The children were categorized as having 1.9 complaints per child. Thirty percent were "aggressive and anti-social" (boys predominating 4 to 1); 22 per cent were "passive and withdrawn" (boys $2\frac{1}{2}$ to 1). This latter finding is similar to Phillips' with respect to autism (106), which was a predominantly male category also.

Differences in interests between boys and girls were found by Tyler (148) to be stronger than the differences between English and American children in these same respects, i.e., English boys have interests more similar to American boys than to English girls. Ages of these groups were 10 and 11 years. The only strong nationality difference found was a consistent

tendency for English children to like fewer things and dislike more things than American children do.

Sex-typing.—Two studies have given attention to the process by which children develop sex-appropriate ways of behaving. Fauls & Smith (40) tested a series of hypotheses that related children's choices of sex-appropriate games and tasks to the accuracy with which the like-sexed parent was perceived as making sex-appropriate choices. Using a picture interview with middle class five-year-olds of both sexes, they found that each sex made more sex-appropriate than inappropriate choices, and that only children made more sex-appropriate ones than did children with older like-sexed siblings.

The *Patterns* data were analyzed with respect to the treatment accorded the two sexes. Parents differed a good deal in the extent to which they believed the sexes were different at age five, and also in the extent to which they thought differences in rearing were called for at that age. In the sample as a whole, the most significant difference in treatment was with respect to aggression; boys were more permissively handled than girls. They were given more physical punishment, however, and were more commonly disciplined by the father. Examination of a subsample of mothers defined as having a strong feeling that the sexes should be treated differently at age five showed that this group, additionally, placed on girls more severe demands for conformity to adult standards.

Hubert & Britton's (64) study, previously referred to, shows that in some areas mothers appeared to be somewhat more attentive to their daughters and enjoyed rearing them more than mothers of sons. Mothers of boys were less strict with them, expected less understanding of rules, allowed more activity, and also reported that their husbands had more responsibility for household jobs and family routines than in the case where the child under consideration was a daughter.

These findings suggest some of the factors that may be producing sex differences. As will be discussed later, in the section on identification, these differentials appear to provide better conditions for the rapid development of identification with the same-sex parent in the girl than in the boy.

Socioeconomic status (SES).—Differences in child development and child rearing that are related to social-class membership continue to interest researchers. Rainwater (108) sought a correspondence between social class and personality descriptions of early adolescents provided by the Szondi test. Angelino, Dollins & Mech (4) compared the frequencies of 10 categories of fears and worries in two large SES groups. Two studies have given attention to educational aspirations as these are related to SES. Haller & Sewell (54) queried a random sample of 500 Wisconsin high school seniors as to both occupational and educational aspiration. There were no differences between rural and urban groups among the girls, but the rural boys had lower educational aspirations. This difference could not be accounted for by

lower intelligence or lower occupational aspiration; the authors concluded that farm youth simply underestimated (in comparison with urban youth) the importance of education in achieving an occupation.

The problem of social mobility was considered by Beilen (10). He used an open-ended questionnaire with two lower SES groups—one upward-mobile by the criterion of intent to go to college and one non-mobile—to determine the extent to which the former were influenced by a conscious willingness to "postpone" gratification, i.e., to approximate sociologists' conventional notion that middle-class people are willing to forego primary and immediate gratifications in order to gain longer-term satisfactions. Beilen did an unusually careful job of content analysis, and reached the conclusion that there was no conscious self-deprivation in the mobile group. On the contrary, according to his report (and his data are quite convincing), the graduating lower-class high school seniors who planned to go to college saw themselves as achieving a goal by so doing, not postponing gratification. After a decade or more of sociological and projective-psychological interpretation of the middle-class as one that suffers deep deprivations, it is reassuring to discover that a researcher in this field can discover that there are conscious positive values to education and middle-class status, as perceived by lower-class youth on the way to middle-class status.

A somewhat similar conclusion is reached by the authors of *Patterns of Child Rearing*. Their sample of mothers ranged from upper-lower-class to upper-middle. The lower-class mothers were substantially more punitive about aggression, less permissive with respect to sex and aggression, and more restrictive concerning household matters. In general, the lower-class mothers were more restrictive, more punitive, less happy in their own marriages, and provided less expectation of ultimate academic achievement for their children. This latter point is of some significance for educational purposes. In the fifth year—kindergarten—the lower-class mothers were already more concerned with the success of their children, academically, but they had less conscious expectation of the children's success.

An interesting and unexpected source of confirmation for findings of less sex permissiveness among lower-class mothers comes from a study by Angelino *et al.* (3). Their purpose was to study types of disagreements between mothers and daughters (the latter of adolescent age) with respect to clothing and grooming. Such types of clothing as formals, shoes, shorts were considered. In the lower-status group there was only one category of clothing over which mothers and daughters differed significantly. This was undergarments, and the mothers were the more conservative.

Race.—In the development of five scales that would serve to diagnose physiological birth-trauma in newborns, Graham, Matarozzo & Caldwell (52) found that two of the scales required different norms for Negroes and whites. The Negro newborns were superior on a vision test and on a test that measured degree of sensory-motor maturation (mainly motor). No race

difference was discovered with respect to pain threshold irritability, or tension. None of the five scales showed any sex difference. These well-designed and validated tests provide a significant set of base-line measures for the study of native differences in newborn infants, and of course offer an important aid to the clinician in determining the trauma status of newborns.

A retrospective study of prenatal and paranatal factors, using birth and hospital records, was done by Rogers, Lilienfeld & Pasamanick (113). Subjects were 1000 elementary school children, from Baltimore public schools, who had been reported by school personnel for behavior problems. There was a control group of equal size. The results showed, in the white group, only one type of behavior disorder related to complications of pregnancy and birth: more children were at school age showing hyperactive and/or confused, disorganized activity. In the nonwhite group there were more pregnancy and birth factors associated with all types of behavior disorders. It is not clear why this race difference should appear in these relationships.

EFFECTS OF PARENTAL ATTITUDES AND PERSONALITY

Another way to discover whether a parental attitude or personality is in fact influential on children's behavior is to take a group of subjects with certain known characteristics—stuttering or colicky children, schizophrenics, or children scoring in certain ways on tests of adjustment—and determine whether relationship with the behavior disorder can be found in parental attitudes. Generally speaking, results from this approach have been negative when the instruments used for the test have been questionnaire-type tests of attitudes or personality. Thus Goodstein & Dalhstrom (50) failed to find differences on Minnesota Multiphasic Personality Inventory scores of parents of stuttering and nonstuttering children. Goldstein & Carr (49) found that mothers of catatonic and paranoid schizophrenics did not differ on an attitude scale of items related to child raising, though the scale had previously proved differentiating between mothers of schizophrenics and nonschizophrenics.

Burchinal *et al.* (21) obtained no significant relationship between parental acceptance and adjustment of children, measured on the one hand by a questionnaire-type scale of acceptance-rejection and on the other by the Rogers Test of Personality Adjustment. Correlation analysis was used, with no association found. This result is particularly disappointing, since the study was unusually well-designed from the standpoint of the sample. This reviewer found two points on which more information would have been desirable in the report of the study: (a) no description was given of rapport existing between the interviewer or tester and his subjects; (b) no scatter-plots of the hypothesized relationships were presented. Correlation analysis may obscure as well as shed light on various types of unusual relationships, and the two variables here are too important to leave in this inconclusive state. Further, when the variables under test involve responses potentially threatening to

the ego-defenses of some subjects, the greatest attention should be given to rapport and the subjects' understanding of the purposes of the request for cooperation. Another report by the authors (57) shows acceptance not related to education of parents in this very well-chosen representative sample. Mothers were more accepting than fathers, but their standard deviation was also higher.

Lakin (80) presents a thoughtfully designed study comparing mothers of excessively crying (colicky) infants with mothers of normal, well-adjusted infants. The two groups were roughly equated on socioeconomic status, education, health, age and birth weight of infant, but not on age of mother. Though both groups had just had their first baby, the mothers of the colicky babies averaged 29 years of age compared to 25 years for the control group. This difference is significant at the .05 level. Here we are just beginning to scratch the surface in clarification of antecedent and consequent variables, but this study provides a good start for further work.

A number of studies have investigated the parental authority behavior of parents of schizophrenics. A particularly well-designed investigation is presented by Kohn & Clausen (78). The subjects were 45 young schizophrenics, in their first hospitalization, with an equal number of controls matched on several variables. All subjects were interviewed and reported themselves on the authority behavior of their parents; comparison of these interview results with the reports made by the parents or other relatives on the case history interviews shows a good similarity. The chief result was that schizophrenic patients, more frequently than normal persons of comparable background, report that their mothers played a very strong authority role and their fathers a very weak authority role. This holds true regardless of sex of the patient or his socioeconomic background, i.e., normal males report different patterns of parental authority behavior from normal females, but schizophrenic males report the same as schizophrenic females. Normal respondents of differing socioeconomic background report differing patterns, but the schizophrenics of different socioeconomic levels give the same pattern.

Two studies from the Mayo Clinic (9, 66) are less tightly designed but have the advantage of concomitant intensive study and treatment of the young schizophrenics and their parents. Statistical treatment of the data is absent, but the authors state that certain types of physical or psychological assault occur too frequently in the series to be fortuitous. These assaults include such things as threats of castration, the mother's threatened suicide, choking, etc. Efforts by Gallagher (44) to show a relationship, in college students, between attitudes of harshness toward children and of authoritarianism and opinionation, however, produced negative results. New Zealand delinquents apparently have much the same sort of relationships as those found in America, according to a study of Mitchell (94) from the University of Otago. Younger (ten-year-old) delinquents in Detroit, studied by Wattenberg (152) did not show as much hostility to parents as older delinquents.

METHODS FOR INVESTIGATING PARENTAL ATTITUDES

A check on the validity of Shoben's Parent Attitude Survey was made by Gordon (51). A 12-day camp program for preschool deaf children and their mothers, all of whom lived continuously together with 13 staff members, provided an exceptionally good opportunity for the observation of parent-child interaction in behavioral, rather than reported, terms. All the mothers filled out the parent-attitude survey before coming to the camp; the staff members rated the mothers, at the end of the session, on categories which make up scores of the survey. While the coefficients of concordance among the ratings were fairly high, the relationship between ratings of observed behavior and scores on the attitude survey was insignificant. Shoben's standardization involved comparison between attitudes of mothers of problem and nonproblem children; the present study suggests that the difference found by Shoben does not carry over to mothers of children normal except for deafness.

Word-association tests have recently found more favor than they have for a long time, and a recent study suggests that sensitivity to children may be indicated in this way. Reith & Dawe (109) started with the hypothesis that an emotionally toned experience (reading a story about a fearful little boy) would affect subjects differently on a word-association test, depending on their sensitivity to children. This proved to be the case, when sensitivity to children was judged by majoring in, and having higher grades in, child development than in other courses.

Brodbeck *et al.* (18) developed an ingenious technique for approaching testing of standards of parental discipline in terms of Riesman's "other-directed" and "inner-directed" tendencies. Although the mothers of this study were relatively low in social status, the results showed them to be more inner-directed than other-directed, according to the technique used here. Discussion with the child was the most used form of discipline, except when the child aggressed against his own family openly and directly. In this case stronger punishment was used. Brodbeck's technique is a promising one for studying changes in mothers' attitudes over a period of time and under different conditions, for example, after being exposed to mass media communications.

Spiegel (132) presents a theoretical article on the resolution of role conflict within the family which may provide useful leads toward the analysis of family relationships. Jackson (65) has developed a system of coding by which the free responses of a group of adults to a series of hypothetical parent-child problems can be classified, and has used the system to present sex differences between reactions of mothers and fathers. When methods of control were classified as to degree of coercion (pressure toward conformity), mothers' suggestions of methods were more coercive than those of fathers. Mothers also vacillated more between mild and severe methods than did fathers. These facts are interpreted by the author as being a reflection of role conflicts involved in playing the role of woman (with the stereotype of warmth and lack

of punishment) with the role of mother (with the greater responsibility for effective socialization of the child).

THE LEARNING PROCESS

To quite a few students of child rearing, contemporary learning theory has offered a beguiling framework and set of systematic principles for the interpretation of the conditions under which home and school experiences produce children's personality qualities. Rather surprisingly, however, such interpreters have relied almost entirely on learning theory as it has derived from animal research. They have done little to expand it with studies of those crucial aspects of learning that can be investigated only in human children, such as, for example, the concepts of primary and secondary stimulus generalization, the process of labelling as a factor influencing discrimination, and the relation of language to other forms of behavior. There has been a substantial increase in studies devoted to such problems during this past year, however, and child psychologists appear to be making good capital from the extension of animal-learning theory into these peculiarly human areas of investigation.

The methodological sophistication of most of this work, stemming from lessons in research design long since worked through by animal psychologists, is sufficient that chief attention can be given to research findings. All the conclusions listed below derive from instrumental learning situations. These differ somewhat from one study to another, but Spiker's procedure for measuring generalization (134) is a good example of the general class. A child stood before a panel in which there was a small window that provided a stimulus light. The child was taught by verbal instruction to pull a lever when the light flashed on. The machine delivered a marble in a chute if the response was a correct one. A certain number of marbles could be exchanged for a desired toy. The child could pull as frequently as he wished, while the light was on, and the measure of response-strength was in terms of operant rate. This method has been used for the study of generalization and discrimination.

(a) When number of reinforcements is held constant, intermittent reinforcement produces greater response-strength, as measured by resistance to extinction, than continuous reinforcement (11).

(b) The extent of stimulus generalization (strength of response to similar but nonreinforced stimuli) is a positive function of number of reinforcements of the conditioned stimulus (134, 135).

(c) The generalization gradient is steeper (i.e., starts higher but ends lower on a dimension of similarity-to-the-reinforced-stimulus) with a larger number of reinforcements of the conditioned stimulus (133).

(e) The generalization gradient is steeper when the initial training is to a bright light than when it is to a dim light; i.e., generalization extends to a wider range of similar stimuli when the initial training is to a weak rather than a strong stimulus (133).

(f) On a visual-spatial similarity dimension, the stimulus generalization gradient increases in steepness from ages 7 to 12; i.e., young children generalize to a wider variety of stimuli than do older children (88).

(g) Learning a common name for a set of stimuli produces greater generalization from the reinforced to the nonreinforced members of the set than not learning a common name (120).

(h) Discrimination is learned more rapidly when discrete labels are attached to the discriminable objects than when no labels are attached or when the mere discriminability of the objects is taught (101). Likewise, when a child has a relational concept (e.g., "middle-sized"), he can learn a task requiring discrimination on the basis of relational cues more quickly than if he has not yet attained that concept (137). A group method for exemplifying this principle is described by Shepard & Schaeffer (121). Calvin and others (24, 25) found concepts "large" and "small" easier than a color discrimination.

(i) The attaching of labels to the stimulus objects (by pretraining) improves performance on a delayed reaction test (136).

(j) When labels are not provided, older preschool children tend to invent their own to a greater extent than do younger children (136).

(k) Direct reward (candy) produces more rapid discrimination learning than token reward (tokens to be exchanged for candy), but has no such effect on later transposition learning (146).

(l) When a specific class of verbal responses (plural nouns) is reinforced (a murmured "good") without a child's awareness that the response is being reinforced, he shows an increased frequency of that response under test conditions. In this instance no child realized that the particular response was thus reinforced (31).

(m) In a simple maze-learning situation, three- and four-year old children show evidence that subgoals become integrated with main goals, and hence obtain increased incentive value (72).

COGNITIVE PROCESSES

The "developmental stage" approach to child behavior continues to suffer critical damage by being subjected to careful empirical test. In the 20 years since Deutsche tested Piaget's notions of children's conceptions of causality, there have been repeated indications that cognitive "stages" are no more successive than are the "libidinal stages" earlier described by Freud. Nor are they independent of the educational and cultural backgrounds of the particular children studied, nor are the ages at which they appear at all stable. Estes (39) tested four of Piaget's propositions concerning the development of mathematical and logical concepts, using Piaget's own tests with 52 children ranging from four to six years of age and of both lower and middle class status. Several of Piaget's conclusions failed of confirmation.

Nass (100), in an ingenious and carefully controlled study of 120 eight- to ten-year old children, found that the frequency with which "naturalistic"

(as contrasted with several varieties of "nonnaturalistic") explanations of physical events was given was profoundly influenced by the form of the questions asked. "Why does" produced a much higher proportion of animistic, supernatural, etc., explanations than "How does." Likewise, clinically withdrawn children showed far more nonnaturalistic reasoning than did normal ones, a fact that indicates the importance of personality (and experience) in determining the so-called "stage" at which a child stands—and the necessity of large groups for purposes of normative statements.

Previous studies have shown that adults quickly learn to respond to a choice-making situation at about the "input probability" ratio; i.e., if one of two possible choices is rewarded 75 per cent of the time, and the other 25 per cent, adults tend to respond with about those proportions to each of the choices. Messick & Solley (90) show that children ranging from three years to eight years follow the same principle, but that when the size of reward for a correct choice is remarkably increased, the seven- and eight-year-old children maximized their successes by choosing the 75 per cent rewarded object 100 per cent of the time, the five-year-olds stabilized at 90 per cent, and the three and four-year-olds remained at 75 per cent. In other words, "probability learning" occurs as early as age three, and at five years there is evidence of adaptive reasoning to maximized reward.

A summary and analysis of research on children's thinking, containing wide coverage of the European literature and nearly 1000 references, has been published by Russell (116). Following Piaget's methods of analysis, Malrieu (85) has examined children's perception of time, and concludes that both intelligence and social institutional factors are influential in this aspect of development.

The influence of reward on perception was examined by Solley & Sommer (130). They presented children with two facial profile drawings, offering a reward in association with one. When the two profiles were fitted together into a single somewhat ambiguous figure, the children reported "seeing" the one which had been rewarded; that part of the total figure was described as happier, brighter, nearer, and as having darker contours. Blum (14) also presents findings showing the influence of value on perception of size.

The clear presence of synesthesia involving colors and tones was demonstrated in a group of 995 elementary school children (125). These results are similar to earlier ones obtained from adults. However, seven-year-old children differed from adults in perception of casuality, according to an experiment by Olum (102) using Michotte's technique. The children were less able, apparently, to break down configurations in time and space.

DRIVE SYSTEMS

AGGRESSION

Catharsis and aggression.—If many different forms of aggression are functionally similar with respect to their drive-reducing properties, then the

expression of any act of aggression is a catharsis that reduces the instigation to other acts of aggression. Feshbach (41) gave one group of children an opportunity to play with toys assumed to be conducive to aggressive behavior—Indian, cowboy, soldier, and pirate equipment; while another group played with neutral toys—trains, circus, farm, and store. Feshbach suggests, with reference to the catharsis hypothesis, that possibly for an activity to have drive-reducing properties, components of the specific drive pattern must be present during or in initiating the activity. Thus, the children in the present instance were interested and curious rather than aggressive before engaging in the play activity. The author reports that he is currently engaged in research aimed at clarification of this possibility.

Siegel (122) has also worked on the catharsis effect. The free play of like-sexed pairs of children in the absence of adults was observed under two conditions: after a highly aggressive cartoon film and after a "matched" nonaggressive film. Play was scored for aggression and for behavioral signs of guilt and anxiety. These scores did not differ significantly for the two conditions. The author suggests that the aggression scores were more directly reflective of habit strength than of drive strength. If this were the case, one would expect consistency between aggression shown in this situation and others. Siegel obtained significant correlations between the play session scores for aggression and teachers' ratings of the children's usual aggressive behavior, and also significant sex differences in the play sessions, both of which support the idea that habit strength aggression was being shown, rather than drive-produced aggression. She suggests that the catharsis phenomenon may appear with relation to frustration-produced aggression but not be relevant when instigation to aggression has been influenced by expression of aggression in fantasy. Another possibility is that the film used may not have been a good one for producing fantasy expression in these children. To accomplish the catharsis effect, it may be necessary to portray situations which are very real and familiar to the subjects.

Zuk (157) used the Rosenzweig test with sixth grade children under three conditions varying somewhat in the restrictiveness of the atmosphere, and found differences in reactions of children classified as normally aggressive or passive under usual circumstances, depending on the social context.

Fantasy and overt expression of aggression.—Specificity of habits is borne out in a study by Kagan (70). He administered a set of specially devised pictures to boys ranging from 6 to 10 years, and scored the stories for five different categories of aggressive content. On the basis of teacher ratings of fighting behavior with age mates, the sample was divided into five groups differing in degree of overt aggressivity. Children who were rated as most likely to initiate fighting behavior produced significantly more fighting themes to pictures than boys rated as nonaggressive; but other categories of aggressive fantasy were not related to the fighting behavior ratings.

Direction of aggression in adult-child interaction.—An interesting new Picture Situations test, somewhat resembling the Rosenzweig picture frustra-

tion series, has been used by Morgan & Gaier (96) with a group of children, ages 9 to 12, and their mothers. Evidently, children and mothers look at the punishment situation rather differently: while mothers direct more aggression onto children than children direct onto mothers, children direct more of their aggression onto themselves than mothers direct onto themselves. This is the normally expected training function of mothers. But, in addition, children conceive of mothers as directing more aggression onto them than mothers conceive of themselves as doing, and mothers conceive of themselves as directing more aggression onto themselves than children conceive of them as doing. Different frames of reference evidently enter the picture to distort the perceptions of both parties to the punishment situation.

Takala & Takala (143) present a very suggestive study of Finnish and American children's responses to the Rosenzweig test. They found clear differences in the direction of aggression, between Finns and Americans, at each age level from 4 to 11. These authors have also interviewed Finnish mothers in different parts of Finland and base a general description of mothers' attitudes on this material; it will be valuable to have more publication from this source.

School classrooms no doubt have their effect on children's learning about approved and disapproved behavior. Meyer & Thompson (91) made direct observations of teacher-pupil interactions in three sixth grade classrooms, each of which had a woman teacher. Boys received significantly more disapproval than girls; they also received somewhat, but not significantly, more praise. *Guess Who* responses from the children showed that they were aware of the greater disfavor of the boys in the room. The authors suggest that the boys receive punishment, from the woman teacher, for behavior which they do not really see as "bad"—i.e., masculine aggressiveness.

An exploratory study of resemblances in aggression patterns between parents and children was carried out by Hess & Handel (60). Fathers and mothers were found to differ strikingly in the way in which they appear to influence aggressive behavior in their children. These findings, based currently on a small sample, deserve much further exploration.

The mother is seen, in Thematic Apperception Test pictures, as the more punitive parent by both delinquent boys and delinquent girls, according to Young (156). Kagan (69), working with younger nondelinquent children, found the mother usually described as more friendly, less punitive, dominant and bossy than the father in the ages six to eight. Eight- to ten-year-olds, however, tended to report the same-sexed parent as more dominant and punitive.

Children selected for retardation in reading show five types of reaction to authority, according to Spache (131). Using the Rosenzweig test, he shows that these children may be aggressive and hostile toward authority figures, or adjusive and inoffensive, or defensive, or solution-seeking, or autistic.

DEPENDENCY

Two major articles dealing with emotional dependence and attention-seeking have been presented by Gewirtz (45, 46). The seeking of attention from an adult, by preschool children doing painting in a room alone with one adult, was first categorized into nine types of behavior: attention-seeking, questions, comments, glances, number of paintings completed, time spent in painting (the child was told he could paint as long as he wanted), praise-seeking, help-seeking, and permission-seeking. A factor analysis of the intercorrelations of these resulted in three factors. This analysis paves the way for investigation of antecedents of behavior which characteristically appears in children's relations to adults, and a program for this is proposed. Cataloguing of instrumental behaviors, reinforcers, and object persons which characterize the dependence process, together with age relationships and interrelationships, constitutes the dimensional program. The developmental program would employ the catalogue to focus upon the etiologies of different emotional dependence patterns. These proposals represent a sophisticated approach to a group of behaviors which have been frequently noted and considered to be of real importance in the systematic analysis of personality, but which have hitherto been imperfectly understood. Possibly now the way has been facilitated for more penetrating developmental study, and the work on emotional dependence may parallel in quality and quantity the investigations of aggression.

SEX, AFFECTION

There have been no studies during the year on developmental aspects of sex drive. Sex-typing has been discussed under demographic variables, and sex-role questions, together with identification, will be considered in a later section on self-other relationships. It is difficult to conceive of a Kinsey interview of young children, but until something like this is done we are not going to know enough about expressions of sex drive to make a start on discovering antecedents and consequents of this presumably important part of children's make-up. *Patterns of Child Rearing* (119) reports that mothers apparently try to prevent sex activity from starting at all, in their young children, by minimizing, in every conceivable way, stimulation that could arouse sexual impulses and thus lead to the strengthening of the drive by experience. Avoidance, by the mothers, of labels for sexual matters seems to have been one rather notable method of achieving this aim. No doubt this tendency also contributes to the difficulty of devising methods for study of sex behavior, since children are probably aware, on the one hand, of the culture's nonpermissiveness with reference to such behavior, and on the other have not been taught (by their mothers!) any positive values or labels with which to communicate their feelings to others.

Walters, Pearce & Dahms (151) studied affectionate behavior, as well as aggressive, in preschool children. At all age levels children were more affec-

tionate than aggressive in their response to others, and more frequently employed affection than aggression in initiating contacts.

ACTIVITY, CURIOSITY, MANIPULATION

Harlow *et al.* (55) demonstrate that manipulatory motives appear early in the life of baby monkeys; by 20 to 30 days of age strong tendencies are observable. This behavior tends to increase with age and experience; no satiation or extinction is observed. The evidence, according to Harlow, "strongly suggests that this is not a derived motive." Certainly these behaviors are very much in evidence in children of almost every age; yet no studies bearing specifically on these systems have appeared during the year.

ANXIETY

A series of articles by Castaneda, McCandless & Palermo (26, 27, 86, 87, 103) first reports the development of a children's form of the Manifest Anxiety Scale, and then its use in testing propositions with respect to the effect of anxiety on learning, intelligence, achievement, and social status of children. The scale was adapted from Taylor's adult form and has been used successfully with fourth, fifth and sixth grade children. It consists of 42 items, for which the "anxiety" response is "yes"; plus 11 lie scale items, for which the falsification response is also "yes." Retest reliabilities were found to be satisfactory. Grade (age) differences were not significant, but sex differences were, in that girls scored higher on both anxiety and lie scales.

Predictions as to the effect on simple and complex learning of differing anxiety levels were made, and the following results obtained: (a) high anxious children tend to be inferior, in comparison with low anxious children, on the difficult components of a learning task, but show a tendency to superior performance on the less difficult components; and (b) high anxious children show poorer performance than low anxious children in a learning situation involving the presence of competing incorrect responses. Both of these results were obtained on groups composed of both sexes, drawn to the experimental groups in approximately equal ratios.

Relationships between anxiety scores on the one hand, and intelligence, or achievement scores on the other, appeared quite clearly negative for sixth grade girls, i.e., with higher anxiety, intelligence scores are lower. Sixth grade boys showed a negative relationship with achievement but not intelligence. Of the various part scores on the achievement test, arithmetic scores were the most strongly related to the anxiety scores. Another study showed generally that the more anxious children tended to be less popular, but here there are grade level differences and some lack of homogeneity in the group.

Two studies, using high grade mentally retarded boys, are related to the preceding. Keller (71) and Callahan (23) have worked with auditory digit span and anxiety.

ACHIEVEMENT

In view of the importance of this drive system, and the extensive work relating to achievement motivation in young men of college age, it is surprising that little has been done on its developmental aspects. Five studies have appeared during the past year, three of them European in origin. Robaye (111) presents an interesting experimental study of level of aspiration and level of expectation in 50 young girls, with Rorschach and other personality data related to combinations of high and low aspiration and expectation. Since these combinations result in small subgroups, the statistical findings need replication in order to assure confidence; however, there are numerous suggestions that the development of values in the family are reflected in the aspiration levels and adaptation to real life circumstances of these girls. An exploratory study by Sivertsen (126) studies the level of aspiration, especially its social determinants, in a realistic social context.

Predictions based on concepts of expectancy and goal value were used in a study by Worell (155). The value of an activity was determined first by ranking procedures, and then coupled with instructions which described one activity as highly important in predicting athletic ability and also important to the experimenter, another activity as less on both, and a third as of practically no importance. Expectancy (estimate of next performance) was significantly lower in highly valued situations. This effect was not altered by the introduction of a penalty for inaccurate estimates.

Bujas *et al.* (20), working at the University of Zagreb, present an experimental contribution to the psychology of competition in public schools. Several groups performed over 12 days under differing conditions of motivation. Greater improvement in the task (addition problems) was found where a small difference in performance was reported as between two teams. When one group was made to fail and the other succeed regularly, both teams declined in output.

Walsh (150) used doll play (the Driscoll Play kit) in comparing self-concepts of bright boys, one group of low achievers, one of adequate achievers. The low achievers consistently differed from the adequate achievers in portraying the boy doll as restricted in action; unable to express his feelings appropriately and adequately; being criticized, rejected, or isolated; and acting defensively through compliance, evasion, or negativism. A hypothesis relating sex identification to achievement was not confirmed.

SELF-OTHER RELATIONSHIPS

IDENTIFICATION WITH PARENTS

Men and women college students, and their parents, comprise the population for a study of identification by Gray & Klaus (53). Responses to a sentence completion test and to the Allport-Vernon-Lindzey Study of Values, filled out by the subject, by his parents, and by the subject as he believed his

mother and father would respond, were used in the study. Similarities between same-sex parents and children appeared in both men and women; however, the relationship was much stronger in the case of the women and most of the predicted relationships held up better for the women than for the men.

Payne & Mussen (104) selected junior and senior high school boys with high and low father-identification scores, measured by the extent to which father and son had responded identically to the California Psychological Inventory as compared to the extent to which mother and son responses were identical. High and low father identification boys were then given an incomplete stories test and judges rated each story for the presence of concrete rewards from and psychologically sound relationships with the parents. A significant relationship was found between high father identification and perception of the father as rewarding and affectionate.

In the study just reported, the measure of identification was taken from agreements on responses to tolerance, social participation, and masculinity-femininity scales. Levin & Sears (81) measured identification in preschool children by a scale labelled "evidence of super-ego in the child"; the child was rated from the mother's description in answer to questions about his behavior when he had done something naughty. High super-ego was judged to be present when the child appeared to feel miserable when naughty, when he always confessed his misdeed and showed a strong need for forgiveness. The reasoning here is that the identification process is necessary to the formation of the super-ego, and that the amount of identification the child has with parental roles can be estimated from the stage of development he has reached in internalized control. Also ascertained from mother interviews were the severity with which the parents punished aggression, and an indication of which parent usually did the punishing. The main hypothesis tested was the following: the more strongly a child is identified with a given parent, the more nearly will he approximate in doll play the level of aggression he perceives as characterizing that parent.

As was predicted, boys who were highly identified and who had the distinctive cues for male aggressiveness provided by being usually punished by their fathers, showed the highest frequency of aggression. Identification was related to high aggression in the girls only when it was associated with severe punishment by mothers who usually did the punishing, i.e., when the girl was identified with an aggressive role model.

High school boys who adopt their families as a reference group show a larger proportion who plan to go on to college, according to Rose (114). Among girls the opposite is true.

Conscience in Swiss and American children was investigated by Boehm (15), using the "méthode clinique," as developed by Piaget. The study appears to give evidence that in certain areas of social development the American child matures earlier than does the Swiss child, apparently transferring his parent dependence to a peer dependence at an earlier age.

Clear sex-role preferences in both boy and girl kindergarten children were shown by Brown (19), although girls somewhat more than the boys tended to accept components of both male and female roles. No relationship was found with socioeconomic status. Brown has developed a sex-role preference scale which should prove useful in future investigations. Faults & Smith's similar study (40) has already been referred to. Bossard & Boll (16) present a theoretical article on child behavior and the empathic complex. Ausubel and others (5) examined the responsiveness of fourth and fifth grade children to adult expert opinion, by means of an art preference test. Emotionally disturbed adolescents show more agreement with overcontrolling restrictive child-rearing practices than normal children of the same age, according to a study by Spivack (138).

SELF-IMAGE

Various forms of social behavior, and anxiety about each, were explored by Child, Frank & Storm (29), using self-ratings, Thematic Apperception Test (TAT), and childhood background measures based on subjects' reports. The self-ratings showed consistent but small correlations with events in the later childhood of the subjects, but no consistency in the relation of these retrospective reports to the TAT measures. A new instrument, designed for high school students, has been constructed by Spivack (139). Self-acceptance and self-rejections are rated. Human figure drawings—same-sex, opposite-sex, and self-figures were obtained, by Silverstein & Robinson (123) from 22 children in the chronic stage of poliomyelitis. While inspection appeared to show that more than three-quarters of the subjects appeared to represent their disability, either directly or indirectly, in their drawings, comparison with drawings of a normal control group failed to distinguish between the two.

SOCIAL INTERACTION

SOCIOMETRIC ANALYSIS

A very comprehensive analysis of sociometric choice status has been done in Sweden by Bjerstedt (12, 13). Six assumptions often involved in interpretation of scores are made explicit and then tested for their validity. In general the results from Swedish children appear to be governed by the same factors as those obtained from American or British children. These two works are excellent sources for methodological consideration in the use of sociometrics.

Dunnington (37) has performed much the same service with reference to sociometric choices among American preschool children. Although the number of subjects used is small, the data are analyzed to investigate a number of points of disagreement in the area of preschool sociometric research. A second study (38) by the same author investigates behavioral differences of sociometric status groups in play sessions. High status children differ from low status children in showing more verbal interaction, both positive expression

and aggression; imaginative behavior does not differentiate the two groups.

Davis (34) has examined correlates of sociometric status in a well-selected sample of eighth grade boys. Low but significant relationships were found between the sociometric rating and IQ, adjustment, pubescence, achievement in reading, and attitudes toward school. No relationship appeared between sociometric status and age, socioeconomic class, over- or under-achievement.

Residential proximity and the choosing of friends was examined by DeVault (36). Generally speaking, children tend to choose friends living farther away as they progress through 12 grades of school. And, sociometric choice methods were used successfully with mentally deficient children by Clampitt & Charles (30). They found that children with mental ages above $3\frac{1}{2}$ were generally able to make satisfactory choices. Sociometric status correlated highly with supervisory evaluation based on selected traits, and MA and IQ were also related.

OTHER METHODS FOR STUDY OF SOCIAL INTERACTION

Moustakas *et al.* (97) present a schedule for observation of adult-child interaction, with a total of 89 adult and 82 child categories, as well as anxiety-hostility ratings. The schedule has been tested and found reliable in studying parent-child interaction in home and laboratory (playroom) and in studying therapist-child interaction. Certain general trends, similarities, and differences in mother-child interaction in home and playroom and in therapist-child interaction in the playroom are noted. Johnson (67) studied participation in block play as an indication of socialization of first grade children.

Patterns of behavior stemming from fourth grade children's responses to a Guess Who questionnaire were subjected to factor analysis by Mitchell (95). Resultant factors were labelled "social acceptability," "aggressive maladjustment," and "social isolation." These suggest variables for emphasis in future research.

"Functional" leadership was studied by Gardner (43) by means of an ingenious new technique. Levitt (84) has re-analyzed data for the Horowitz Faces test, concerned with anti-Negro prejudice in children, and finds that validity of the test is unsatisfactory. Reliability is found to be poor for one method of scoring, though adequate for another.

SOCIAL INTERACTION IN SELECTED GROUPS

Thorpe & James (147) investigated the effects of context of the testing situation on scores of a great variety of tests for normal and neurotic (receiving psychiatric treatment) children. Previously differentiation of scores had been found when neurotic children were tested in a clinic setting and normal children in a school setting. The present study removed this differential by testing all children in a mobile caravan brought to the school grounds. Both neurotic and normal children were told they were chosen for the testing by having their names drawn out of a hat. Tests were used which

had previously distinguished the two groups, when the different settings for the testing were used. On this occasion most of the previous findings were not substantiated. Verbal tests of self-report type (sentence completion, worries, etc.) showed no difference between neurotic and normal children, performance tests (leg persistence, dynamometer persistence, etc.) showed nonsignificant differences in the predicted directions. The one exception to the negative results was in sociometric tests. There was a significant tendency for neurotic children to be less popular than their normal controls.

Truants and juvenile delinquents were compared for social relationships in a study by Croft & Grygier (32). Results showed that truants had extremely few friends; delinquents had a few friends but also very many enemies. The authors concluded that truants were, on the whole, more socially maladjusted than delinquents, since the delinquency appears to be a kind of defense mechanism to reduce social isolation and to find acceptance at least among outlaws.

An interesting study of speech patterns in a group of schizophrenic children showed that their speech deviated from the expected normal in a number of ways—rhythm, intonation, articulation, etc. Such inadequacy makes, no doubt, for difficulty in communicating mood. Affect may or may not be flat, but it is clearly poorly communicated. This study was done by Goldfarb, Braunstein & Lorge (48).

ABILITIES

INTELLIGENCE

Prediction from infant testing.—Earlier in this review the work of Wittenborn (153) on a study of adoptive children has been cited with reference to the effects of environmental conditions on children's development. Part of this study involved comparison of scores made by the children on the Yale Developmental Examination of Infant Behavior, at the ages of between 12 and 40 weeks, with Stanford-Binet and other types of scores at age either 5 years, or 7, 8 or 9 years of age. Wittenborn's monograph contains a wealth of reasoning about difficulties in selecting a proper sample for this type of comparison; his data partly derive from years before when practices at the Yale Clinic of Child Development were geared to social needs in the examination of babies for adoption rather than the obtaining of a representative sample of the total population. The author has worked through the past records and set up various subgroups which check, to a certain extent, on sampling error. The reasoning with respect to choices of groups of children to include and to contrast is a model for other researchers on this difficult topic to emulate. Briefly stated, the results as obtained indicate unreliable and extremely small relationships between scores on the infant examination and later intelligence tests. Where selective placement, possibly on the basis of judgments of placement agencies endeavoring to match parents and adoptive infants, was probably used, the correlations were somewhat better but still

not consistent enough to make prediction possible. Subjects involved in this study numbered close to 200, and the data are probably as good as any which could be gotten on a *post hoc* basis.

A study by Cavanaugh and others (28) on 34 children who were tested every 6 months from 6 to 60 months of age, gives much the same result. In this case the tests used were first the Cattell Infant Intelligence Scale and, after two years of age, the Stanford-Binet. The latter scores were higher and statistically different from the Cattell mean scores. Prediction from the six months scores to later intelligence scores was poor.

Simon & Bass (124) present a well-reasoned argument for classifying intelligence scores in broad categories rather than using IQ or other scores divided into smaller units. Using three categories of intelligence, they obtain a *P* value of less than .001 for correlation on 56 cases tested in infancy with a retest by age five. The same *P* value was obtained with another group of 46 children tested between ages 5 and 20, with each age pretty well represented. As in the studies previously mentioned, the scores are mainly at above average and superior levels of intelligence, and the authors suggest that correlations would be larger if the sample were better distributed.

Longitudinal studies of intelligence, achievement, personality, and physique.—Bayley (7) in her presidential address to the Division of Developmental Psychology of the American Psychological Association discusses the problem of prediction in development. With the excellent data collected over many years through the Berkeley Growth Study, she illustrates individual patterns of development in physical growth, including rates of maturing, and in intelligence. Her concluding statement is worth quoting:

The more one goes over these cases, the more evident it becomes that the pattern of growth in each child is unique. We can compare him with his peers, and with his own past history. Often, after the fact, we can find an explanation of the causes of his deviations, some of which seem to be inherent and some environmental. With the accumulation of this kind of information we can hope gradually to develop general rules that will permit us to make some evaluations and some tentative predictions, and even, in some instances, to correct undesirable deviations.

Another article by Bayley (8) shows that intelligence, as measured by repeated tests, increases from 16 to 18 to 21 years of age. All categories of intelligence (performance, verbal) showed some increase in scores with age, but there were differences in age at which a ceiling appeared to be reached.

Schoonover (118) reports a longitudinal study of sibling resemblances in intelligence and achievement. Jones (68) reports on later careers of boys who were judged, during adolescence, to be early- or late-maturing. Currently these "boys" are approximately age 33. Physical differences, so prominent earlier, have tended to disappear in adulthood. Personality characteristics show still a number of significant differences—e.g., higher scores for the early-maturing on measures of "good impression" and "socialization." Where such differences were found, they tended to describe the young adults much as they had been described in adolescence.

A large number of high school students in Helsinki were examined over a three year period by Viitamäki (149). Personality traits, ability, and achievement were investigated. Comparisons are made between scores at puberty and, three years later, as the students were well into adolescence. Predictions with reference to matriculation examinations and constancy of scores were examined. Constancy was in general rather low for personality traits. Kent & Davis (73) have studied the relationship of intellectual development to discipline in the home.

VERBAL ABILITIES

Rogers (112) performed a factor analysis on tests of adolescent boys and girls, aimed at discovering personality traits related to verbal fluency. General verbal ability was found to be related to desurgency—subjects reported themselves to be gloomy, worrying, shy. Tallent (144) found high behavioral control to be related to arithmetic, similarities, and series tests, while a poor control (impulsive) group did better on picture completion and digit symbol. They were not better on word fluency.

Howie (63) found accuracy negatively related to speed, when ability was held constant. Speed appeared as a rather broad factor in a variety of test media, indicating an individual trait of tempo in functioning.

Three studies are directly related to language development. Sampson (117) found language related to emotional atmosphere of the home, provision of toys, books and material stimulus, and example and encouragement to speech. Smith (127) presents a study of the relation of word variety and length of words to chronological and mental age in preschool children and material from women past middle age. Rudisill (115) compares advanced and retarded readers in the third grade on flashed digit and phrase recognition.

PHYSICAL GROWTH AND CONSTITUTIONAL FACTORS

Davidson *et al.* (33) found a number of significant relationships between physical constitution and personality traits in seven-year-old children. Kretschmer's (79) classic book on body type has a 22nd edition, with a new chapter devoted to "constitutional developmental physiology." Michael *et al.* (92) find a poor prediction from clinic classifications of male children as "introverts" to incidence of schizophrenia in the subjects at age 26.

Holden (61) administered the World Test to handicapped and nonhandicapped children; found few clear differences except for weaker ability, in the handicapped, to establish self-protective defenses and more concern with destructive fantasies. Hood (62) reports on a boarding school for brain-injured children.

TEXTBOOKS

During the year, we have had reports from conferences on child development (56, 128, 129, 145) and several texts for use in child development courses (99, 140, 141). Books using the case study approach include one on

normal children (93), one on teacher-child interaction (98), and one on emotional disabilities (42). A book for teachers on meeting children's emotional needs also appeared (35).

SUMMARY

The studies reported in the foregoing pages do not show any one characteristic distinguishing the year 1956-57. There are, however, a number of studies which will take an earned place in a respectable science of child development. These studies are well designed to test important questions, and have provided answers which, while not simple, are solid and illuminating to the total field. Within the current framework of expressed theory and methodology, it seems clear that very simple relationships will not appear.

Children are tough and are not going to be influenced alike by what appear to be similar environmental effects. Moreover, evidently the cultural matrices surrounding, for example, being a boy child and being a girl child are strongly different, so much so that studies in the area of personality development show sex differences practically without exception. Partly this may be due to faulty measurement of an environmental influence; e.g., a "warm" mother may express her warmth differently toward boy and girl babies. Since most mothers are opposed, consciously, to differential treatment to two children, the mother may not be the most accurate reporter of her own behavior in this regard.

When environmental influences are properly measured, no doubt the principles operating in the child's development will be seen to be the same for both sexes as well as for children differing in other respects. This is an example of the kind of complexity which, it appears, is going to require increasingly very careful preplanning by researchers in order that the design can take care of the important sources of variance. This year's studies, for the most part, show understanding of this and therefore represent a sound contribution to development of an area of knowledge.

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LEARNING^{1,2}

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The major theoretical contribution of the year has been the publication of *Behavior Theory and Conditioning*, the Silliman Lectures given by Spence (139). Its most radical aspect is the treatment of reinforcement. Hull (68) had postulated that any modicum of reinforcement was sufficient to give a full increment to habit strength (sHr). The amount of reinforcement was only important because of its influence on incentive motivation (K). Spence carries this trend to its logical conclusion in the case of instrumental reward learning. He distinguishes between classical conditioning, in which the response strengthened is the same as that evoked by the reinforcement, and instrumental conditioning, in which it is quite different. He assumes that reinforcement is necessary for classical conditioning but that contiguity is probably sufficient for instrumental. The basis for this change to a contiguity position is his interpretation of incentive motivation. The vehicle for K is the fractional antedating goal reaction (r_g). It is the intensity of the implicit stimuli produced by r_g that determines the drive properties of K , and this adds to the traditional drive of Hullian learning theory. Thus, reinforcement is necessary because it evokes and determines the characteristics of r_g ; this in turn provides a major source of motivation for instrumental behavior learned on the basis of contiguity. In this way a new two-factor theory of learning is born. As Spence indicates, it is time for Tolman to take the far side of the bed.

This change in emphasis is likely to play havoc with the remaining Hullian constructs. With attenuation of the role played by primary reinforcement, it is doubtful if there is need or justification for giving as much weight as formerly to the concept of secondary reinforcement. Stimuli that formerly acquired secondary reinforcement properties should now be the ones that control r_g and thus indirectly motivation. Similarly, the stress on the contiguity principle for instrumental learning invites, although it does not demand, a reinterpretation of extinction effects in terms of counterconditioning. Conditioned inhibition, sIr , would seem more appropriate to classical conditioning and the extinction of r_g ; the resultant decrease in motivation would permit the emergence of new instrumental responses necessary for counterconditioning.

¹ This review covers the period from April, 1956, to April, 1957.

² The following abbreviations and symbols are used in this chapter: sHr (habit strength); K (incentive motivation); r_g (fractional antedating goal reaction); S (subject); PGR (psycho-galvanic response); CR (conditioned response); CS (conditioned stimulus); UR (unconditioned response); US (unconditioned stimulus); Ir (reactive inhibition); sIr (conditioned inhibition).

Interest in statistical learning theory continues unabated. This year it has invaded the Continent (8), and its phylogenetic reach has included small children (94) and gold fish (23). To acknowledge this maturity, other such studies will be reviewed in terms of the variables they investigated rather than be segregated in a special section.

At the same time the situation most thoroughly studied by the statistical learning theorists, the Humphreys guessing board, continues to attract alternative interpretations. Both Simon (135) and Edwards (38) conclude that the relative frequency with which S picks each of the two alternatives is best predicted by the principle of regret, a principle borrowed from statistical decision theory. Furthermore, Edwards feels that the behavior of S is guided by two sets of hypotheses, large ones determining strategy, e.g., one response pays off more frequently than the other, and small ones determining tactics, e.g., the exact order for shifting between responses. As these latter are never consistently confirmed in a random situation, they are continually changing. It is this variability with which the statistical learning theorist is dealing. Unfortunately these approaches to the problem by Simon and Edwards are very limited; they deal only with asymptotic behavior, they say nothing about rates of adjustment in the situation, nor are they readily generalized to other arrangements.

Other theoretical contributions range from a classified bibliography of over 600 Russian studies on salivary conditioning in dogs (119) to an ambitious attempt to integrate the concepts of learning theory and those of ethology (144).

STIMULUS AND RESPONSE

Stimulus.—The psychologist's use of the term stimulus remains vague. The experimentalist uses it at the common-sense level to designate the objects and events he manipulates. For the theorist, however, it is a construct implicitly defined by its relationships with other terms in his system. The only common property it seems to possess for different theorists is that of being a cue, the basis for differential responding. Presumably if he were to complete his system, the theorist would provide a set of rules coordinating his construct with the common-sense objects and events of the experimentalist. These rules would constitute a miniature perceptual system characteristic of each theory.

The divergent directions these coordinating rules might take are illustrated by the controversy between exponents of continuity and those of noncontinuity in discrimination learning. One assumption of the continuity position is that all aspects of the sensory input, occurring at the time of a reinforced response, acquire a cue function. Three recent studies have been interpreted as favoring this position. Babb (9) used two pairs of stimuli, A-B and C-D, in a simultaneous discrimination with the choice of A always reinforced. For three different groups of rats, C occurred with A either 30, 50, or 70 per cent of the trials. The groups were tested on the C-D pair alone

with C positive. The first group showed negative, the second zero, and the third positive transfer. The implication is that any stimulus aspect differentially reinforced acquires a cue function even though it is not the primary cue in the situation. This finding was confirmed by Jeeves & North (73). The fact that the 50 per cent group in Babb's experiment showed zero rather than negative transfer is of interest to the continuity theorist. It suggests that S does not learn to ignore or adapt to irrelevant stimuli that are present during discrimination learning. As Kelleher (78) points out, this result is predicted by the continuity interpretation. The noncontinuity theorists have not offered any experimental rebuttals to these findings.

Still the question remains as to how one enumerates the various aspects of a stimulus situation. Are the rectangularity of the choice chambers, the tactual feel of the floors, the smell of the wood all aspects of the situation in the sense of becoming associated with the response? If so, each acquires a positive habit strength as S perfects the discrimination. At the same time each is common to the two alternatives. The result is that the number of common aspects so outweighs the differential ones that the discrimination should soon break down.

Estes (42) is one of the few continuity theorists who has faced up to this issue. The complexities he finds are illustrated by two studies. Burke, Estes & Hellyer (22) trained three groups in a verbal conditioning situation, each with a different degree of stimulus variability. To account for the differences in rate of learning, it was necessary to assume that 80 per cent of the cue function was due to constant background stimulation common to the two alternatives. Then Schoeffler (131) ran a second study using a very similar stimulus situation, but first training his Ss to discriminate between two patterns of lights. Upon testing them on various combinations of the two patterns, he had to make the implicit assumption that the common background stimuli had zero influence in order to account for his results. To reconcile these two studies, one must assume that the establishment of a discrimination in Schoeffler's study did suppress the cue function of the background stimuli.

The difficulties in handling the concept of stimulus generalization are similar. As presently formulated, the main problem is to find a rule for ordering stimulus situations that will predict the amount of transfer between them. The use of the term *similarity* is somewhat suspect because its response-inferred nature may well cover a variety of different psychological processes. A suggestion to this effect is found in a study by Rothkopf (125). He took 36 Morse code signals and paired each with the remaining 35. These pairs were presented auditorally to Ss who judged them to be either the same or different. The mean number of same judgments given to a signal was used as an index of its average similarity to the rest. This index gave high positive correlations with the number of substitution errors made to each signal during code learning. Thus it would appear that this index of similarity accounts for the interference due to stimulus generalization during learning. On the

other hand when he paired each of the 36 signals with itself, the index based on judgments of sameness showed high negative correlations with substitution errors. In this case it is doubtful if the concepts of similarity and stimulus generalization are even appropriate. Rather it suggests that when a stimulus is so ambiguous or complex that it cannot be retained in memory long enough to be recognized immediately thereafter, S will have considerable difficulty in associating any consistent response with it.

Even psychophysical scaling, the blood-brother of similarity, has difficulty in providing an ordering rule for stimulus generalization. In the case of the pigeon, Guttman (58) reports that for variations in hue the generalization gradient takes its most simple form when expressed in terms of the physical wave length of light rather than in terms of psychophysical units.

A related problem occurs in attempts to predict the influence on performance of combining two stimulus situations each of which has been independently conditioned to a response. Sometimes this facilitates but on other occasions inhibits performance. In favor of facilitation, Grings & O'Donnell (55) found that a combination of two independently reinforced lights yielded a larger PGR amplitude than did a combination of a reinforced and a nonreinforced light. The interpretation of these results is complicated, however, by the rapid extinction of the PGR during testing. More ambiguous are the results reported by Kalish & Guttman (75) working with generalized responses. They trained pigeons to peck at each of two colored lights which differed by a fixed number of wavelengths. The assumption was that this would produce two overlapping generalization gradients. The birds were tested on a range of wavelengths spaced along the spectrum. The resulting response rates were compared with those obtained when only one color had been conditioned. The data fitted either of two hypotheses about generalization gradient interactions equally well, or equally badly: (a) there is no additive effect of two gradients, and (b) the gradients add exponentially.

Kalish & Guttman suggest that their results probably apply equally well to a study of humans by Bilodeau, Brown & Meryman (13). These investigators used a row of seven lights, the Ss having been instructed to respond as rapidly as possible to lights 3 and 5 but not to the rest. The generalization gradient was based on responses to these prohibited lights when they were unexpectedly introduced into the sequence of reaction-time trials to the two permitted lights. The frequency of response was greatest to prohibited light number 4, and this frequency was greater than was expected from studies using only one permitted light. This was interpreted as showing the additive nature of generalization gradients.

To voice a criticism of increasing popularity, this type of reaction-time study, with the emphasis on the control of performance through instructions, probably falls outside the boundary conditions stipulated by theories postulating interaction between generalized habit strengths. Nonetheless, this procedure has been used to relate stimulus generalization to the age of children (93), to clinical anxiety (124), and to previous experience (122).

What role to assign past experience in determining the stimulus aspects that acquire a cue function remains an important though not a clearly answered question. Both Gibson & Walk (50) and Forgas (47) have demonstrated that rats who have been visually exposed during early development to various geometrical forms, such as triangles and crosses, learn to discriminate between them more readily than do control animals. In addition the study by Forgas suggests that the earlier this experience is given the greater is its beneficial effect. These studies indicate learning without obvious reinforcement or a clearly defined overt behavior. Whether or not the learning involved was at a perceptual level specific to the geometrical forms used cannot be determined. Adequate controls that would have ruled out more general forms of transfer were not employed in either study.

Response.—The term response has a vagueness equal to that of stimulus. Experimentalists agree that it is a class concept defined in terms of achievement, e.g., depressing a bar or breaking a light beam. At the same time it is a theoretical construct. The difficulty is in relating the two in any consistent manner. Clearly there is a need for a set of coordinating rules that would be the complement of a perceptual theory.

The problem becomes most acute in transfer studies as Nissen has indicated (104). The type of transfer a theory predicts depends as much on its definition of a response as on its classification of stimulus situations. This is the source of the continued interest in response versus place learning (33, 48). These studies indicate that the definition of a response that best predicts transfer effects depends in part on the type of stimulus situation involved. Variegated external stimuli favor a place concept whereas a reduction in these favors a response concept.

A second source of interest in this problem comes from the progressive changes in behavior during learning. Boguslavsky (14), for instance, describes such changes in leg flexion during avoidance conditioning, and tries to account for them on contiguity principles. A more subtle change in response with training is found in a study by McKelvey (87). Rats, run on a simultaneous discrimination with a correction procedure, at first adopted a fixed position habit when leaving the starting box. If this led to the positive stimulus, they continued; but if it led to the negative, they corrected themselves at that point. In later stages they tended to make a choice before leaving the goal box. This latter behavior appeared sooner with a noncorrection procedure. While not tested, the suggestion is that whether the response is best described as an approach tendency or a right or left turning tendency depends in part on the stage of training observed.

Statistical learning theory, however, has established one point about response classification. The usual procedure in the Humphreys guessing situation is to define two mutually exclusive and exhaustive categories of response, A and B. Both Neimark (102) and Schipper (130) have found that the asymptotic performance of response A tends to be independent of the number of other possible response categories used.

Stimulus-response correlation.—A further difficulty in defining stimuli and

responses arises from the possibility that they may be interdependent. There is the continued suggestion that the stimuli *S* reacts to depend in part on prior responses he has learned. This shows up in the formulations by Reid (120) and Wyckoff (154) on the acquisition of observation behaviors. Indirect support for these formulations is offered by Pubols (118). He found that overlearning on a black-white discrimination led to faster reversal even after the elimination of all position preferences. The interpretation is that overlearning permits the acquisition of observation responses which limit the effective cues to the relevant ones in the discrimination. A complementary suggestion comes from Goodnow & Pettigrew (51). When their *Ss* had to learn a definite pattern of choices between two alternatives, such as an alternation pattern, the rate of learning depended upon the previous responses the *Ss* had learned in that situation. If they had learned an orderly sequence of moves, they readily noted and remembered how the alternation pattern deviated from their own choices; otherwise they did not.

The converse possibility, that the stimuli available in a situation determine the response class adopted, is less well documented but nonetheless a possibility. As indicated previously, studies on place learning suggest that whether the behavior is best described as place or response depends in part upon the extensiveness of the extramaze stimuli provided. Pastore (108) also urges a reconsideration of the possible relationships that can exist between stimulus and response classes. He presents a theoretical argument against the idea that these can be independently defined with the expectation that learning then can form an association between any pair of them.

DRIVE

The concept of drive is intimately related to the distinction between learning and performance. It has been traditional to think of the sophisticated organism as having a number of learned behaviors each associated with a given stimulus complex. There is no assurance, however, that this stimulus complex will evoke its associated behavior unless *S* has a characteristic internal state called a drive, e.g., hunger or thirst. Hull (67) formalized these ideas in terms of his multiplicative law that performance is the product of drive and habit strength. Drive in this conception is referred to as an "energizing" factor. Two implications are involved: (a) when drive strength is of zero amount, there is no evocation of behavior, and (b) the greater the drive strength the stronger the performance as measured by its persistence, its speed, and its vigor.

Measurement.—A perennial question in this formulation has been how to measure drive. The usual assumption has been that it is monotonically related to the number of hours of food deprivation, amount of sex hormone injected, or the intensity of the electric shock applied. Equally well, however, one should be able to infer it from measures of performance, assuming that prior learning has been controlled. Frequently these two measures of drive strength disagree, especially when a consummatory response is used

in measuring performance. For instance, when an animal is placed on a fixed deprivation schedule, its food intake only gradually stabilizes over a long period (46). Furthermore, the continued use of a fixed deprivation period results in a "rhythm" effect. Intake is maximal at the end of this period, and any lengthening of it, which presumably should increase drive, reduces the intake (83). A comparable result has been found by Cautela (26) in discrimination learning. After they had learned the problem under a fixed deprivation interval, various subgroups of rats were extinguished on a range of intervals. Resistance to extinction was maximal for the training interval and decreased for either shorter or longer ones. To account for these discrepancies between deprivation and performance measures, it is usually assumed that the deprivation measure correctly mirrors drive strength; the performance measure is always confounded with changes in habit strength produced by assumed variation in internal stimulus conditions.

A more serious problem for the Hullian concept of drive is its implicit assumption that all types of drive have comparable effects on performance measures. Miller's recent review of his studies on this problem suggests the complications involved (96). He used three measures of performance: (a) rate of bar pressing, (b) amount of reward consumed, and (c) the threshold of acceptance for a reward when a noxious taste has been added. He induced hunger or thirst drives by a variety of procedures such as deprivation, injection of saline solution, or the injection of drugs. The three performance measures proved to be differentially sensitive to each of these methods of inducing a drive. Miller concludes that either drive does not have a unitary influence on the various performance measures, or else a whole series of side effects must be postulated for each method of inducing a drive state.

Stimulus intensity concept.—Among theorists using the drive concept, the search has been for some property common to these internal states other than their influence on performance. The popular candidate at present seems to be the intensity of the stimulation assumed to accompany each drive state. Once stimulus intensity is accepted, however, all stimulus situations, whether internal or external, give rise to a drive in some degree. This is formally recognized in the concept of stimulus-intensity dynamism, which was introduced to account for the negative correlation between reaction time and the intensity of stimulation.

Whenever an intense signal is introduced into a reaction-time experiment, it usually contrasts sharply with the other background stimuli. Consequently the question with regard to stimulus-intensity dynamism is whether or not it is due to intensity per se or to a contrast effect. In one study Heyman (65) trained rats to run to either a dark or a light gray. Subgroups of these were then extinguished on five different grays ranging from very dark to very light. He found little evidence that intensity influenced performance either during learning or extinction. His apparatus, however, tended to minimize contrast effects. This was rectified by Johnsgard (74)

whose rats learned to run to one of five grays, ranging from dark to light, with each presented against a large midgray background. The running speeds formed a U-shaped distribution with the fastest times for the darkest and lightest grays. When subgroups were extinguished on each gray, the same U-shaped distribution was found. Thus, it seems clear that contrast has some drive function. It must be admitted, however, that the range of visual intensity investigated has been relatively small.

Stimulus intensity also plays a role in the concept of incentive motivation as this is formulated by Spence (139). It is assumed that the stimuli resulting from r_0 provide part of the drive for instrumental behavior. Clearly any attempt to demonstrate this relationship will have to be indirect with reliance placed on the manipulation of the characteristics of the reward. Such attempts are discussed below under the concept of reinforcement.

This search for a common property in all drive states can never keep pace with the multiplicity of new drives postulated. Not only is there continued interest in exploratory behavior as indicative of a drive state (43, 152), but now a frustration drive enters to complicate matters. While Hull (68) and Spence (139) assume that the removal of a reward weakens incentive motivation, Adelman & Maatsch (1) claim it produces a new drive called frustration. This is an emotional reaction. Behavior learned in order to reduce it can become fixated and relatively impervious to extinction. Whether or not this latter claim can be substantiated, Amsel & Hancock (4) offer evidence that behavior immediately following a nonreinforced response is more vigorous than when following a reinforced one.

Acquired drives.—The concept of acquired drives has been most useful in accounting for avoidance conditioning. At present the main concern in this area is whether or not a two-factor theory of learning is needed. According to Church, Brush & Solomon (30) the experimental evidence favors such a formulation, with one set of principles governing the acquisition of the acquired drive and another set controlling the instrumental behavior based on that drive.

Now Kimble & Dufort (80) attempt to give acquired drives a prominent role in classical conditioning. They found that after the first few CRs had been elicited in eyeblink conditioning, Ss could be given a series of trials with the airpuff alone, and the acquisition curve would increase as rapidly as it did for a control group having the CS and US paired on every trial. Their interpretation is that the associative factor in conditioning occurs very rapidly. Additional trials are needed only to establish an acquired drive based on the US. This drive is necessary to evoke the CR on test trials. Unfortunately this study was repeated with additional controls by Goodrich, Ross & Wagner (52) and their experimental group failed to improve during the unpaired trials. They attribute the Kimble-Dufort results to temporal conditioning due to the use of a constant intertrial interval in that study.

A phenomenon closely related to the behaviors found in acquired drive studies is that of behavioral fixation resulting from insoluble discrimina-

tions. Maier (89) has reviewed most of the studies in this area and has reiterated his faith in their compulsive nature. Perhaps some additional light is thrown on the problem by Ellen's study (40). He used rats who had established a spatial fixation on a two-card discrimination and tested them when there were three cards to choose between. The jumping stand was so placed that either one or two cards appeared on the rat's fixated side and the remainder on the non-fixated. With only one card on the fixated side, the rat almost invariably jumped to it irrespective of whether it was positive or negative and irrespective of the combinations of cards on the nonfixated side. With two cards on the fixated side, however, the rat had a marked tendency to pick the positive one, both when it was directly in front of him and when it was considerably to one side. This variability in jumping behavior on the fixated side suggests the following formulation. The rat shows a compulsive avoidance of the nonfixated side, not a compulsive reaction to the fixated one. This change in emphasis explains nothing, but it points up the similarities between this behavior and that found in avoidance conditioning.

Drive and learning.—The acceptance of drive as an energizer, and of the multiplicative relationship between drive and habit strength, commits one to a number of implications about drive interaction and about the shape of the traditional learning curve. One implication is that two drive states should have an additive effect on performance. Some support for this is offered by Hall (59). When he added relatively intense buzzers to the rat's environment, there was an increase in its wheel-turning activity. This could be further augmented by food deprivation.

A second implication is that an increase in drive can either facilitate or hinder performance during learning depending upon which response in the individual's hierarchy has the greatest habit strength. If the correct response is first in the hierarchy, high drive should facilitate performance, but if an incorrect one is first, high drive should hinder the acquisition of the correct behavior. The Iowa group has shown the most energy in investigating this implication. Drive level is manipulated indirectly by selecting Ss on the basis of their responses to the Taylor Manifest Anxiety Scale. The rationale is that this test either measures a prevailing state of drive or else it measures the individual's reactions to stress of the type he experiences in learning situations.

Taylor (143), after reviewing 49 studies, decides that whatever this individual-difference score is measuring, it acts like a good drive should. In addition Spence *et al.* (140, 141) contribute two studies on paired associates learning that contrast the performances of high and low anxiety groups. They attempt to control the position of a response in the hierarchy by manipulating the associative value between the stimulus and response term making up a pair. To control the relative strengths of correct and incorrect response tendencies, they manipulate the intralist similarity between stimulus terms. Both studies tend to support the differential prediction concerning the relationship between drive level and performance.

Franks (49), on the other hand, questions the first prediction that high drive facilitates performance of the response highest in the hierarchy. He reasoned that if the Taylor scale is measuring drive only, unconfounded by other variables, then the same prediction should hold for the rate of eye-blink conditioning among unselected Ss, half of whom had been deprived of food, water and tobacco for 20 hr. and half of whom had not. He could find no supporting evidence. This negative finding probably will not carry much weight in deciding the issue. Nonetheless, this direct manipulation of drive seems more appropriate to the theory involved than does the indirect selection of Ss by a paper and pencil test.

Greater consideration has to be given to an objection raised by Saltz & Hoehn (127) concerning the second prediction, that involving response competition. They point out that many previous studies have confounded response competition and level of difficulty, the latter being taken as an indication of the former. In an attempt to unconfound them, they constructed three lists of syllables for serial learning that were not significantly different in difficulty for low anxiety Ss as measured by the number of trials to criterion. The first consisted of syllables of 90 per cent association value and high intralist similarity, the second of 13 per cent association value and moderate similarity, and the third of zero per cent association value and minimum similarity. They argue that if intralist similarity correlates with response competition then high anxiety Ss should do more poorly than low anxiety Ss on the first list but better on the third. Instead, the high anxiety Ss were only slightly poorer on the first list and significantly poorer on the third.

The prevalence of this tendency to confound difficulty and response competition is attested by two other studies (25, 61) attempting to extend the Iowa predictions to externally induced drives. These are difficult to interpret because they postulate implicit but unmeasured response competition to explain their results both when the anxiety groups differ as predicted and when they do not.

Another issue concerning the role of drive strength during learning is whether it only influences performance or in addition modifies the strength of the associations involved. The preferred experimental design on this question is one in which several drive levels are used during learning, the drives are then equated, and the Ss tested for resistance to extinction. Eisman, Asimow & Maltzman (39) used this design on a discrimination problem with the number of perseverative errors during discrimination reversal taken as the measure of resistance to extinction. This measure correlated positively with the drive level during learning. However, they allowed only 48 hr. between the end of learning and beginning of extinction, which is probably insufficient to ensure that the drive levels were equated. A different approach was used by Straughan (142) within the context of statistical learning theory. He attempted to induce two different drive levels in his groups by using either a loud or soft tone that the Ss could escape by responding. This vari-

able had no clear effect on either theta value, i.e., the index of learning rate, or the asymptotes reached.

The relationship between drive strength and learning is made more complex by the fact that all learning theories assume that each change in drive level produces changes in the internal stimuli accompanying the drive state. These stimuli must be part of the complex associated with a response, for only thus can these theories account for the discriminations *S* makes between his various drive levels and various drive states. This implies, however, that each change in drive causes a response decrement due to stimulus generalization.

Manning (92) has made a fairly extensive study of these assumptions by pitting various levels of the hunger and thirst drive against each other in order to determine which dominated the choice made in a discrimination situation. After training rats to choose one side of a T-maze when food was removed for 11 hr. and the other when water was removed for an equal time, he tested them on all 16 combinations of 0, 11, 23, and 35 hr. of food and water deprivation. The stronger of the two drives tended to dominate the choice. This was true even when both test drives were stronger than the training one. This, however, is contrary to what one would expect from a literal interpretation of stimulus generalization. The complicating fact is the interaction between hunger and thirst drives; hungry animals are not likely to drink nor thirsty ones to eat. Consequently, it is difficult to specify the relative strengths of the two drives used in this study.

REINFORCEMENT

Reinforcement has to be defined in relationship to some state of the organism, for in the absence of that state the same event which previously increased the probability of response may no longer do so. This has encouraged two assumptions, first that the state involved is a drive as defined in the previous section, and secondly that reinforcement consists of a reduction in this drive. This second assumption should have led to a demonstration that all means of satiating drives can act also as reinforcers. Instead, the much more specific hypotheses have been investigated that (a) drives are either need states or forms of intense stimulation, and (b) that reinforcements are either based on need reduction or decreases in stimulation.

Drive reduction.—Chambers (27) finds evidence for the need reduction hypothesis even when consummatory behavior is not involved. His apparatus permitted him to inject automatically various solutions into the veins of rabbits as they moved freely about an enclosed area. The experimental *Ss* received a sucrose solution whenever they entered a nonpreferred section and saline upon entering any other. The control *Ss* received xylose, a sweet tasting but nonnutritive sugar, and saline under comparable circumstances. The experimentals spent increasing amounts of time during a training session in the originally nonpreferred sections whereas the controls did not. Chambers admits that there may have been taste factors accompanying the in-

jection of these solutions and even body temperature changes (28). Nonetheless, these results give a role in reinforcement to need reduction even in the absence of any pronounced consummatory behavior.

On the other hand, both the consummatory and need reduction factors are stressed by Miller, Sampliner & Woodrow (97). Using rats with fistulas in their stomachs, they first trained them to bar press for water. Then after a uniform thirst drive had been established, these Ss were given a fixed amount of water either by direct injection or by being permitted to drink normally. The resulting changes in drive level were inferred from the subsequent rates of bar pressing or of drinking additional water. These measures indicated that both groups had a lower drive level than did control Ss, but that this reduction was greater with normal drinking than with injection of water into the stomach. These experimenters conclude that direct need reduction is important, but that this is supplemented by oral factors that act as secondary reinforcers. This is fortunate because it permits the latter to bridge the time gap between ingestion and digestion.

Activity on the stimulus intensity reduction front continues unabated. This year adds to the number of studies showing that animals will press bars to turn off an electric shock (36) or a loud noise (11). However, light intensity is a different problem. Hurwitz (70) reports that his rats will learn bar pressing when it turns on a weak light but not when it turns out the light and leaves them in the dark. The restless monkey, on the other hand, gladly presses a bar just as long as it makes some change (99).

Perhaps of more concern to the stimulus reduction theorist is the collapse of one of his more dramatic illustrations. When human Ss are given a continuing shock, their hearts first accelerate and then decelerate. Zeaman & Wegner (155) had found that in conditioning heart rate a two-second shock, which terminated during the acceleration phase, tended to produce CRs of this type, but that a 6 sec. shock, which terminated during the deceleration phase, tended to produce CRs of the latter type. Not being able to leave well enough alone, they extended these studies to include shock durations of two tenths of a second and 15 sec., intervals so short and so long that in terms of their previous findings the heart should have been beating normally when they terminated (156). Thus, there should have been no conditioning. Unfortunately the results were strictly comparable to those of the original study. After reviewing all of the data, the authors concluded that the form of the CR was not dependent upon what was occurring at the time of shock termination. Instead, accelerative CRs were characteristic of Ss giving large URs and decelerative ones of those giving small URs.

At an empirical level there is a continuing interest in scaling the reinforcing value of different stimuli (12, 24, 29) and in discovering new types of reinforcement (64). Perhaps inevitably, drive and reinforcement properties seem to be totally confounded in many of these studies.

Secondary reinforcement.—So long as experiential factors enter into the definition of reinforcement, as is the case with secondary reinforcement,

opponents of the drive reduction hypothesis are in for a tough time. This seems to be the present predicament of Sheffield & Roby. They had demonstrated that saccharine, a sweet-tasting but nonnutritive substance, acted as a reinforcement for hungry rats (132). On this basis they had argued against the need-reduction hypothesis. Now Smith & Capretta (136) offer fairly convincing evidence that the saccharine effect is due to secondary reinforcement based on need reduction. Their rats were given a saccharine solution either 2 hr. after eating, while digestion was still continuing, or else 21 hr. afterwards, when digestion was complete. Subgroups had either 4, 8, or 15 such experiences. They then were trained on a T-maze with saccharine on the nonpreferred side. All groups learned, but the 2 hr. groups showed decreasing errors with increasing numbers of pretest experiences whereas the 21 hr. groups showed increasing errors. The authors conclude that the taste of saccharine gained reinforcing power the more it had been associated with primary need reduction; it lost whatever power it initially had when experienced in the absence of need reduction.

Despite the important theoretical role played by secondary reinforcers, the empirical demonstrations of their influence on learning frequently have been disappointing because of the transitive nature of the changes they produce in behavior. It is of interest, therefore, to find two studies reporting quite sensitive indices. The first by Miles (95) contrasted the amount of spontaneous recovery shown in the presence of a secondary reinforcer with that shown in its absence. Two groups acquired equal bar pressing habits. During the first period of extinction, group A received a secondary reinforcement for each bar press while group B did not. During the second period of extinction, these treatments were reversed, and so on. An influence due to the secondary reinforcer was still detectable during the fifth extinction period. Because of the strict alternation between conditions, however, it may be that the group taking longest to extinguish during one period showed less spontaneous recovery during the following period. The second study by Powell & Perkins (117) added a secondary to a primary reinforcement in a discrimination problem. Two degrees of secondary reinforcement were first established, one for each of two end-boxes. The hungry rat was then given a choice between them when each contained food. A preference rapidly developed for the end-box with the greater secondary reinforcement.

It is even harder to find clear-cut demonstrations of secondary reinforcement associated with stimuli accompanying shock reduction (10). Nefzger (101), for instance, persisted through three experiments of the following design. Rats learned to escape shock by running into one of two distinctive goal-boxes. They were tested on a T-maze whose stem was similar to the training apparatus and with one of the goal-boxes at each end. There was no evidence that they learned to prefer that box which presumably had acquired secondary reinforcing power during preliminary training. Possibly these negative results could be due to either of two factors. Assuming that the rats experienced anxiety in the T-maze stem, escape into either box

would be rewarding and this would mask the effects of prior training. It is even more probable that the rats developed a rapid discrimination between the training and test situations as suggested by Murphy & Miller (100). These experimenters attempted to get higher-order conditioning in monkeys by using a variety of CSs, shock as the US and bar pressing as the CR. While several Ss showed second-order conditioning, only one out of thirteen showed third-order. The authors emphasize that at each level the lower-order CS continued to evoke a response. Thus, the failure was not due to extinction effects. Rather, the monkeys seemed to discriminate readily and clearly between those trials on which the CS was preceded by a new stimulus and those on which it was not.

Once secondary reinforcement is demonstrated, there is still the question whether or not responses reinforced in this way show the same properties as do those based on primary reinforcement. A study by Brown (18) raises this question in conjunction with Hull's multiplicative theorem for drive and habit strength. After pairing light and buzzer with food for the experimental rats but not for the controls, she put half of each group on high and half on low drive. All were trained on bar pressing using only light and buzzer as reinforcers. The superiority of the experimental group demonstrated the effectiveness of the secondary reinforcers, but the lack of any interaction with drive level was contrary to the Hullian expectation. Any prediction of this sort has become highly tenuous if one follows Spence's formulation concerning incentive motivation. If the light and buzzer become associated with r_p , their influence merely adds to the primary drive level. Instead of the rate of bar pressing indicating a learning process, it is an index of drive level. It will be a tricky experimental task from now on to distinguish between secondary reinforcement and incentive-motivation effects.

Consummatory responses.—Consummatory behavior continues to attract attention both because of the assertion by Sheffield, Roby & Campbell (133) that it provides the reinforcing effect and because of Spence's conception of its role in incentive motivation. There can be little doubt that it has an influence over and above that attributable to the need reduction that usually accompanies it. For instance, Kling (81) ran thirsty rats on a runway while varying both the length of time they were permitted to drink and the size of the opening in the drinking tube. Their speed of running bore little relationship to the amount of water ingested, but it was highly correlated with the amount consumed per unit of time. This suggests that it is the vigor of the consummatory behavior, not the degree of need reduction, that is the crucial variable. It is quite possible that such analyses of behavior accompanying drinking will begin to throw light on the differential performance effects found when the concentration of sucrose solutions is varied (32, 137, 138).

In the case of the amount of a food reward, its positive correlation with the speed of running has fitted the incentive motivation hypothesis. This interpretation has been less secure for the rate of learning a discrimination

when the amount of reward is varied. Lawson (84) suggests a possible reason for this. In his study where rats were learning simultaneously two equally difficult discriminations, the absolute quantity of reward made little difference in learning rate when both discriminations contained the same amount. However, if a given animal received a large reward on one of these and a small one on the other, fewer errors were made on the former. It was as though a contrast effect were involved.

In this year of the little r_o 'ses, when they are used to account for motivation, reinforcement, extinction, and discrimination, attention should be called to an important theoretical analysis by Deutsch (35). He points out that Hullians frequently have confounded confirmation of an expectancy with reinforcement of a response. This is most apparent whenever r_o is postulated. For instance, assume that a rat has an r_o based on food, and that while this response is being elicited the rat encounters water and drinks. This would disconfirm his expectation insofar as this is defined by his r_o characteristic of food. Nonetheless, the drinking should reinforce this r_o even though it is an inappropriate reward. Once this is granted, as Deutsch points out, r_o can not be used as a source of differential stimulation to explain *umweg* behavior, problem solving, or latent learning as it has been in the past. Nor, as he shows, does one escape from this confusion between confirmation and reinforcement by postulating that an r_o based on food is incompatible with one based on water. His analysis makes clear that the response properties of this omnipresent explanatory device and their relationship to reinforcement must be clarified. This is especially urgent in studies such as Pereboom's in which a specific r_o is postulated for each amount of food consumed (109).

Partial reinforcement.—Besides the type and amount of reinforcement, the schedule of reinforcement must be taken into account in specifying the persistence of behavior. Present attempts to explain the increased resistance to extinction generated by partial reinforcement divide on the question of whether intertrial or intratrial factors are of most importance. The Sheffield hypothesis (134) emphasized intertrial factors, i.e., the aftereffect of a nonreinforcement became part of the stimulus complex for the succeeding trial. With Weinstock's demonstration (151), however, that partial reinforcement effects obtained even with 24 hr. spacing between trials, the swing was toward intratrial factors. It is assumed that on a nonreinforced trial S learns something, such as how to tolerate frustration, that prepares him for the extinction series.

Katz (76) ran rats in order to contrast these two viewpoints. His Ss alternately ran a black and a gray runway, with 20 sec. between the black and gray trials and 15 min. between the gray and black ones. Half the group had partial reinforcement on the black and continuous on the gray, whereas the other half had the converse. All were extinguished on the gray. The second group with partial reinforcement on the gray showed the greatest resistance to extinction, suggesting that stimulus traces from an immediately preceding nonreinforcement were not as important as were experiences of

nonreinforcement in the actual extinction situation. The crucial variable is the experiencing of nonreinforcement, not merely uncertainty about the outcome of a trial, as has been shown by Kendler *et al.* (79). When they rewarded every trial, but presented the two rewards of food and water in random sequence, there was no evidence of a partial reinforcement effect. Tyler (146), on the other hand, emphasizes that the nonreinforced experience must occur in the same stimulus context as do the extinction trials.

Nonetheless, intertrial factors undoubtedly play some role in producing greater resistance to extinction when training trials are massed. For instance, Grosslight & Radlow (56, 57) have found that the inclusion of nonreinforced trials which are immediately followed by reinforced ones, during the learning of a discrimination, retarded the reversal of that discrimination. On the other hand, the inclusion of such trials did not have this effect if they came at the end of a training day with 24 hr. intervening before the next reinforced trial.

Partial reinforcement also has become a problem for statistical learning theories. Both Atkinson (6) and Neimark (102) introduced blank trials into their training series, i.e., trials on which S was not given any information as to the correctness of his choice. This was an analog to partial reinforcement. Atkinson concluded that stimulus elements on such trials do not become associated with either response, whereas Neimark assumed that they become associated with the response made on that trial. This contradiction is more apparent than real because Neimark's procedure did not permit her to distinguish between the two possibilities. On the other hand, Atkinson's hypothesis has the unfortunate implication that S should continue performing indefinitely at the asymptotic level even though the experimenter has turned off the reinforcing lights and gone home for the day.

It is doubtful if any single assumption of this type can account for the range of hypotheses Ss establish as a result of the instructions given them or the context of the situation. A good example of this is found in a study by Lewis & Duncan (85). Using a real slot machine and nickel payoffs, they gave eight training trials and then an extinction series. The group that did not receive any reinforcement during training took over 100 trials on the average to extinguish. As the reinforcement ratio during training was increased for other groups, resistance to extinction decreased. Clearly this anomalous result must hinge on the discrimination S made between his previous experience with such gadgets and the behavior of the present one.

Delay of reinforcement.—There is agreement that any delay in reinforcement slows down acquisition (72). The theoretical question is whether this variable determines the maximum habit strength obtainable or whether it only inhibits performance. The former hypothesis is favored by Harker's results (62). He found that the introduction of a delay, after a habit was once established, did not produce a performance decrement. But if the habit was established with the delay present, reducing this delay did result in improved performance.

At present, however, the main interest in the delay variable concerns its effectiveness in increasing resistance to extinction. The trend is to relate it to partial reinforcement by conceiving of a nonreinforcement as an infinite delay. These two procedures were compared by Peterson (110). He used a 2×2 design with continuous vs. partial reinforcement and immediate vs. variable delayed reinforcement. Immediate and continuous reinforcement were additive in increasing the speed of running during learning. On the other hand, partial and delayed reinforcement were additive in increasing resistance to extinction. The factor common to these latter two procedures was apparently the amount of time the rat spent in the goal box while reward was unavailable. Logan, Beier & Kincaid (86) try to explain how this experience could be effective in building up resistance to extinction. They postulate a discrimination involving r_θ and its accompanying incentive motivation. With short delays of reinforcement, r_θ is evoked prior to the occurrence of the reward and is partially extinguished. With long delays, S learns to inhibit r_θ in the presence of these prereward stimuli and thus to protect it from extinction effects. The authors then demonstrated that a variable delay of from zero to 9 sec. did not increase resistance to extinction whereas one of from zero to 30 sec. did. Presumably the first interval was too short to permit a discrimination. It is not clear, however, how this hypothesis would handle Fehrer's study (44) which showed that being delayed in the goal box after reinforcement was almost as effective in increasing resistance to extinction as a delay before the reinforcement.

EXTINCTION

Massed practice on motor skills consistently results in performance decrements. These are in part temporary as shown by reminiscence effects, but this recovery in performance level is rarely sufficient to account for all the superiority of spaced practice. Consequently, investigators in this area have tended to favor a two-process theory involving I_r , a temporary decrement due to responding, and sI_r , a more permanent one. These remain among the most vague concepts in the Hullian glossary (147).

Performance decrement.—Jahnke & Duncan (71) estimate that it takes at least 24 hr. for all I_r to dissipate. This estimate is based on a contrast between a massed and spaced group after practice on the pursuit rotor. Both groups showed reminiscence after a 10 min. rest, but only the massed group did so after a 24 hr. period. The authors argue that I_r must still have been dissipating in the massed group at the end of the 24 hr. in order to counteract the forgetting shown by the spaced group. If this indirect measurement of I_r dissipation is accepted, it complicates the interpretation of previous studies that have investigated the concept of sI_r . These have assumed that 10 min. of rest, as a rule, corrects for any accumulated I_r .

An ingenious attempt to analyze the influence of massed practice on performance is reported by Archer & Bourne (5). Their apparatus measured both the time S spent writing each letter and the time between letters during

the copying of an inverted alphabet. Massing lengthened both of these measures. There was no evidence, however, that the resting time between letters became longer with continued massed practice, as one would expect if S were learning some type of resting response in order to permit the dissipation of *Ir*.

A secondary question about performance decrements concerns their specificity. Evidence is accumulating that they appear in a fairly wide range of behaviors other than the specific one practiced. This is demonstrated by studies, such as the one by Albright, Borresen & Marx (3), in which these decrements show bilateral transfer. It is conceivable that they are due to some form of stimulus satiation rather than to the actual elicitation of an overt response. For instance, Dember & Millbrook (34) have visually exposed rats simultaneously to a gray and a black (or white) alley without permitting them to enter. Then both alleys were made either black or white. The resulting preference was for the alley showing the greatest change in brightness. It was as though the Ss had become satiated with and were avoiding the stimuli they had been exposed to. Duncan (37), after reviewing studies on *Ir* and neural satiation effects, concludes they are quite similar and that both are central phenomena.

Among those who accept the response interpretation of *Ir*, it has been assumed that the amount of work per response is an important determinant of the rate at which *Ir* accumulates. To demonstrate this demands that one be able to separate out the influence of this variable on *Ir* from its influence on learning. This is a somewhat hopeless undertaking as suggested by the complexities in Aiken's results (2). He varied the work load, as defined by the amount of weight acting against a swinging door the rat had to open, during learning, extinction, and spontaneous recovery. The work factor made for a significant difference during learning, but not during extinction, and then showed up again during spontaneous recovery.

Trotter (145) has criticized studies of this sort on the grounds that the variable manipulated, i.e., the weight of the bar or panel, is not a good index of the work done by the rat. To overcome this he devised a rotating bar that gave a continuous record of the force expended at any moment. He found that during practice the amount of effort per reward gradually was reduced until it did correlate with the weight attached to the bar. But this correlation was attenuated rapidly during extinction. The rat frequently pressed with maximal force as though emotional and motivational states were involved. Thus, the work demanded per response probably influences not only *Ir* but also the nature of the response learned and the motivational states involved.

Planaria dorotocephala do not show *Ir* effects but at the moment of writing it is not clear what ramifications this will have for the construct (121).

Latent extinction.—The permanent decrements in response resulting from nonreinforcement have usually been attributed to *sIr*, a concept closely related to *Ir*, and, therefore, to the actual elicitation of behavior. Recent

demonstrations that these extinction effects can be produced by placing the rat directly in the goal box without food and without requiring it to go through the instrumental behavior leading to the goal have led to a search for alternative explanations. The present substitute is r_0 and its role in either incentive motivation or secondary reinforcement. Experimental support for this switch from overt to implicit behavior is offered by Moltz & Maddi (98). After learning was complete, they placed groups of rats, each with a different level of hunger drive, directly into the goal box without reward. The amount of latent extinction shown increased with the drive level involved. The authors reason that the stronger the drive present the more vigorous the r_0 accompanying this drive, and that this leads to its more rapid extinction. But if r_0 extinguishes, there is no incentive motivation (or secondary reinforcement) to maintain the instrumental behavior. The Brown & Halas (19, 20) studies can be interpreted similarly. When the nonreinforced experience occurred in a goal box similar to the training goal box, it resulted in less extinction effect than when it occurred in a box identical to the training one. Due to stimulus generalization, r_0 was not elicited as vigorously and, therefore, did not extinguish to the same degree in the former case.

VERBAL LEARNING

Free association.—Since the time of Galton there has been a strange fascination in the fact that an adult, when presented with a word, can give an associated word in reply. The present interest is no longer in the idiosyncratic associations but rather in the culturally common ones. The assumption is that the manner in which these associations are organized for the individual depends upon previous learning and thus largely on cultural usage. Hall & Ugelow (60) demonstrate this relationship by taking high and low frequency words, as defined by cultural usage, and measuring the association time of Ss to these words. Both the average latency and the relative number of different associations were greater for the low than for the high frequency words. The high frequency words led to stereotyped associations that were readily evoked. A somewhat different technique was employed by Rosen & Russell (123). Using their own standardization of the Kent-Rosanoff words as stimuli, they had each S give two associations to each word of a sample. They found that the first association given tended to have a higher cultural frequency than did the second. About 25 per cent of their Ss, however, violated this rule.

Assuming that the ability to remember a word is related to the ease with which it evokes associations, it is possible to use this relationship in controlling the rate of rote learning. For instance, Noble & McNeely (105) first scaled a set of nouns in terms of the average number of associations given to each in a set time. These were used to form lists of paired associates containing all possible pairings of high and low association values for both stimulus and response terms. The average association value of a pair proved to be the best index of how rapidly it was memorized. Similarly, Mandler &

Huttenlocher (91) have found significant negative correlations between trials to criterion in rote learning and the association values of their scaled nonsense-syllables.

Perhaps of more interest is the influence of these association values upon the order of recall. There is a tendency for the words most readily remembered to occur first in recall irrespective of the order in which they were experienced. This is shown by Bousfield, Cohen & Silva (15). After reading a list of words to a group of Ss, they asked the Ss to write them down in the order they came to mind. There was a high negative correlation between the frequency with which a word was recalled and the average rank order of its occurrence. A second study suggested that even more complex relationships between words control the order of recall (31). A list of words consisting of examples from five categories, such as animals, countries, etc., were read in random order. Recall, however, showed a clustering effect with words belonging to the same category tending to occur in sequence. In the past, part of this clustering has been interpreted as due to mediated generalization. The most direct support for this idea has been those studies showing positive transfer between two word lists consisting of second-order synonyms, i.e., synonyms of synonyms. But Maltzman & Brooks (90) have been unable to repeat these results.

Verbal reinforcement.—Related to the question of which word associations an individual possesses is the problem of how to change the relative frequency of their occurrence. The preferred technique at present consists of giving verbal reinforcers. The only defining property of these seems to be that they are either some unlikely verbalization, such as saying "mm-hmm," or action, such as tapping a pencil, that the experimenter correlates with one class of the subject's responses and omits for all others. If these control the frequency of various associations, then clearly the subject's behavior can be controlled by a variety of subtle, unnoticed interactions with the experimenter. That this is probably the case is suggested by continuing reports that these reinforcers have increased the frequency of either pro- or anti-attitudes during an interview on a social issue (66), increased the number of self-acceptance statements on a personality test (106), and prevented a decline in the quantity of verbalization during the description of a picture (88).

The theoretical issues are whether or not Ss are aware of these reinforcers and their function and to what extent this awareness influences the results. In an extensive study on the first part of this issue, Wilson & Verplanck (153) used a variety of reinforcers and of response classes. The effectiveness of these reinforcers was shown by increasingly frequent use of a response class when they were present and a decreasing usage when they were omitted. The authors report that practically all Ss were aware of the reinforcing event and most were able to report the response class involved. When this was the case, it led to self-imposed instructions to comply.

In an attempt to answer the second part of the issue, i.e., whether or not

the effectiveness of the reinforcer depends upon awareness, Ericksen & Kuethe (41) employed shock as a negative reinforcer. Ss gave free associations to a list of words, five of which were followed by shock. If on succeeding trials these five critical associations were again given, the Ss were shocked until a criterion of two successive trials without any repeats of the critical associations was reached. When the Ss were divided into those aware and those unaware of the reason for the shock, no differences were found in the rates of learning. Nonetheless, the aware group tended to show longer association times to the critical words, during which time they consciously suppressed the shocked associations. Taken in conjunction with the Wilson & Verplanck study, this suggests that awareness probably does make for differences in what is learned and that these differences could be demonstrated by appropriate transfer, generalization and like tests.

Incidental learning.—It has long been known that a group given specific instructions to memorize, irrespective of whether these are self-administered as in verbal reinforcement studies or given by the experimenter, will learn more rapidly and recall more items than will a control group given identical exposure to the material but no instructions. One factor controlling this contrast between intentional and incidental learning is the relationship between the orienting task and the material to be learned. The orienting task is that period during which both groups are exposed to the material and respond to it in some way, such as encircling the items or making free associations. The critical variable is that the intentional group has been instructed to memorize the items involved whereas the incidental group has not been so instructed. Both Saltzman (128) and Postman & Adams (114) agree that some orienting tasks minimize the influence of the instructional variable. This can be due either to the fact that the responses made during the orienting task interfere with the learning of the intentional group, or the task leads the incidental group to make responses very similar to those characteristic of intentional learners while memorizing. The second of these studies, however, suggests that the influence of the orienting task is of greater importance when the items to be memorized are nonsense syllables than when they are meaningful words.

A second factor controlling the effectiveness of instructions is an individual difference one. The amount of incidental learning shown by S tends to correlate with his ability to maintain a set for multiple aspects of the stimulus and with the availability of his responses to it (112). Additional evidence for the influence of set is offered by Postman, Adams & Bohm (115). Intentional Ss were instructed merely to learn the items with the implicit understanding that the order in which these items occurred was not important. When tested for their recall of both the items and the serial order, the superiority of these Ss to incidental learners was considerable on the items but only slight on the order. A second intentional group, instructed to remember both, increased their superiority on order but could do this only at the expense of their recall for items. An intriguing hypothesis, although a difficult

one to test, is that both incidental and intentional Ss learn equal amounts; instructions merely insure that the material learned corresponds to the response measures used by the experimenter. The blinder-effect our response measures have in leading us to narrow conceptions of what is learned is nicely illustrated by the surprise engendered by a recent finding with paired-associates learning. It was discovered that Ss established response-stimulus associations even though they had been instructed only to make stimulus-response ones (45). Apparently, prior investigators had never quizzed their Ss on this aspect of their learning.

Postman & Adams (113) also have used the contrast between intentional and incidental learning in studying the traditional proactive and retroactive inhibition paradigms. They reasoned that the intentional group should make a large number of associations during learning but that some of these would be of low associative strength. The few associations made by the incidental learners, on the other hand, should all be relatively strong. Assuming that weak associations can be interfered with readily, they expected: (a) little difference in retroactive inhibition between the two groups when the intentional instructions were given during original learning, (b) more retroactive inhibition for the intentional group when it was given these instructions during interpolated learning, and (c) greater proactive inhibition for the intentional group only if it was given these instructions both during original and interpolated learning. The results tended to support these predictions.

Rote learning.—Köhler (82) has emphasized that the establishment of associations depends upon a number of relationships between the items other than that of temporal contiguity. Evidence offered for this contention consists in part of the faster learning of homogeneous than of heterogeneous pairs of items. This evidence has been questioned by Postman & Riley (116). They point out that in Köhler's pairs there was never an unambiguous rule for the heterogeneous pairs which told S the type of response to be associated with that stimulus, e.g., that when the stimulus was a number the response was a syllable. In the case of homogeneous pairs, however, such a rule always was available (they consisted of number-number, syllable-syllable, or figure-figure pairs). When the homogeneous and heterogeneous pairs were equated in this respect, Postman & Riley could find no difference in the rates of learning. This supports the contention that it is not the homogeneity of items per se that is the crucial variable, but it leaves the problem of how to incorporate the concept of set into a stimulus-response analysis of rote learning.

The concept of set also is used to explain away another Gestalt phenomenon, the von Restorff effect, in which an isolated item in a homogeneous list is better retained than is a nonisolated one. Green (53) attributes this to a surprise factor based on the impossibility of predicting this sudden change in the characteristics of the series from any rule developed during past experience with it. He has found that when there are two equally isolated items, one in the first half and the other in the second half of the list, only the

first item shows the von Restorff enhancement. Having experienced one such change, S is prepared for the second. Green agrees that this result could be predicted by the concept of proactive inhibition.

The most ambitious theoretical enterprise of the year in serial rote learning has been Atkinson's development of a statistical model to account for serial position effects (7). This model demands the estimation of at least five parameters from the data. But once this is done it can be generalized to lists of any length and can specify the relative number of each type of error made at a given position rather than just the total number.

Retention.—The long cherished belief that the Ebbinghaus forgetting curve is due primarily to retroactive inhibition produced by extra-laboratory learning has been seriously undermined by Underwood (148). His substitute is proactive inhibition. This switch is based on a review of a large number of studies on rote learning in each of which mastery was to one perfect trial, the Ss had been tested for recall 24 hr. later, and no formal interpolated learning had been given. From these data he plotted the percentage of items forgotten against the average number of prior lists the Ss had learned. The percentage increased continuously from 25 for zero previous lists to 75 for ten or more lists. To make things worse, he estimates that when corrections are made for the retention losses due to the low level of mastery, there is only about 15 per cent of the forgetting which can be attributed to retroactive inhibition. This interpretation not only makes proactive inhibition an important variable to control in any study of rote learning, but it leaves us with a pretty paradox. Taking into account learning set phenomena, one must conclude that practice on prior lists simultaneously makes for faster learning and for faster forgetting. Perhaps something of this sort is what Harlow & Hicks (63) had in mind when they suggested that discrimination and learning set data can be accounted for by merely postulating the inhibition of error-making tendencies and without postulating the strengthening of any positive associations.

Despite the niggardly role Underwood assigns it in studies of forgetting, retroactive inhibition retains its importance as a paradigm for transfer effects. Osgood (107) correlated the data from a number of these studies in order to construct his retroaction and transfer surface. This surface relates stimulus and response similarity to the type and amount of transfer observed. In an extensive study, Bugelski & Cadwallader (21) have criticized some of the implications of this surface. They agreed that in paired associates learning there tends to be retroactive facilitation when the responses remain the same and only stimulus similarity is varied between the original and interpolated lists. This facilitation decreases as the similarity between the two sets of stimuli decreases. They disagreed with Osgood, however, in the case where the stimuli were constant and only response similarity was varied. They obtained maximum retroactive interference when the original and interpolated response terms were quite similar, and this interference decreased as the similarity between responses decreased. This result fitted the

Skaggs-Robinson hypothesis much better than it did Osgood's surface. The authors make it clear, however, that the argument centers around the definition of similarity, and until there is agreement on this point there can be little agreement on the prediction of transfer effects.

In addition to stimulus and response similarity, an important variable determining the magnitude of retroaction effects is the relative amounts of practice on the original and interpolated tasks. Briggs (16) studied this variable using lists of paired associates which had the same stimulus terms but different response terms. His results indicated that the amount of retroactive interference was positively correlated with trials on the interpolated list but negatively correlated with trials on the original list. He was able to account for a considerable part of the interference in terms of overt intrusions between the two lists. The intrusion of responses from the interpolated list into the relearning of the original one first increased and then decreased with continued practice on the interpolated material. This supports the idea that interference is maximal when the two lists are equally mastered; when mastered to different degrees, S is able to discriminate between them and thus control the amount of interference.

Rothkopf (126) argues that the intrusion of responses from the interpolated into the original list should be more pronounced than the intrusion of original responses into the interpolated list. This appears to be saying that retroactive influences on forgetting should be greater than proactive ones. His argument is based on the assumption that stimulus generalization results in both sets of stimuli becoming associated with the responses of the other list. In the case of interpolated stimuli, however, these erroneous associations are inhibited during the interpolated learning. This does not occur for the original stimuli. His evidence is based on the successive learning of two lists in which the stimuli were Morse code signals and the responses were letters. In a test of immediate recall where the combined stimuli were given in random order and S was instructed to give an immediate association, many interpolated responses were given to the original stimuli. One wonders, however, how much these results may have been due to a recency effect, i.e., with the interpolated responses well in mind, they were given whenever S had to guess at an association. In any case, the trend decreased considerably in a later test.

In addition to these sources of interference between tasks, intralist factors such as the similarity and meaningfulness of the items composing the list control forgetting to some extent. Underwood & Richardson (149) have some evidence that forgetting results from the interference produced by the recovery of intralist generalizations with the passage of time. They found that the decrement in retention was greater for high than for low similarity items when meaningfulness was low. The effect was less apparent with more meaningful material. Another intratask factor producing forgetting depends upon the many associations that are made between the list items and irrelevant aspects of the situation (54, 111). Any variation in these latter between

learning and recall produces a decrement in retention. But, despite all these possible inhibitory processes, there still is considerable retention of paired associates even after a year when this is measured by a savings score (103).

The stimulus response analysis of memory concentrates almost exclusively on quantitative changes in retention. Perhaps for this reason the Gestalt hypotheses about qualitative changes, such as trends toward closure, symmetry and the like, have attracted renewed interest. Two studies (17, 150) report some support for these trends despite large individual differences. When Saul (129), however, used a recognition test to study changes in remembered symmetry, he found only a central tendency effect, i.e., S tended to pick an item closer to the middle of the recognition series than was the exposed item. In a similar study on closure, Hunter & Duthie (69) could get either a closing or opening of the remembered gap in a figure depending upon the type of interpolated figure S saw between the original exposure and the recognition test. The general conclusion must remain that the influence of any possible autochthonous factors is slight and variable. When, however, the initial stimulus impression is weak, the recognition of that item is readily modified by a variety of interpolated experiences. A suggestion as to how an individual can supply his own interpolated material is given by Kay & Skemp (77). When they interpolated a recall test between the initial exposure to a stimulus item and a recognition test, accuracy on the latter was decreased. If S made an error in recall by either omitting or adding aspects to his recollection of the stimulus item, these imported aspects seemed to block correct recognition at a later time.

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EDUCATIONAL PSYCHOLOGY¹

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INTRODUCTION

Previous contributors to this series have found it necessary to begin with a definition of "educational psychology." In many respects, this may be one of the more important observations about the present status of this field of psychology. Compelling and heuristic definitions of the roles, functions, and content of educational psychology are still wanting and consensus on these matters is achieved only at rather ambiguous levels of abstraction. Some may argue that this concern over definition is not necessary. The reviewer is persuaded otherwise. Attempts to improve the status of educational psychologists in both psychology and education and efforts to up-grade graduate programs in educational psychology appear to call for a clearly defined range of roles and a clear rationale for these roles. Be this as it may, the reviewer of a field needs a definition of some kind even if the field can do without it. The definition used for the purpose of this review has been introduced in general terms elsewhere (1). It is anchored in the conception of education as an explicit effort to bring about desired and specified behavior changes by the manipulation of environmental variables (curriculum, facilities, materials, teacher responses, etc.). The activities of educational psychology are thought of as comprising the attempts to generate and evaluate educational hypotheses; the content of educational psychology is thought of as consisting of the results of such activity and the systematization of these results into more powerful formulations and more effective evaluative procedures. The activities of a given educational psychologist at any given time may not and need not fall within this definition. He operates *qua* educational psychologist when and in the degree to which his attention is directed towards education.

Such a definition of the field is strongly petitioned for in the latest year-book of the Association for Supervision and Curriculum Development (2) and has been argued for, during the period of review, by Calonghi (3) and Derbolov (4). It is well exemplified in the research of Kapos, Mech & Fox (5, 6), who derived and tested some theory-based reinforcement hypotheses about the learning of routine school tasks under school conditions. Mednick & Lehtinen (7) provide another example in their test of a theoretically derived hypothesis (Spence's stimulus generalization) in the context of the elementary school. The Vassar College continuing research project in higher education is a further noteworthy illustration. There, Sanford and his

¹ This review covers the 12-month period ending in April, 1957. Because of the lag in publication of the *Journal of Educational Psychology*, the articles appearing in the February-April issues of that journal could not be included.

colleagues (8) are conceptualizing the problems of higher education, formulating relevant research hypotheses from sociopsychological theory, and testing these hypotheses in the context of higher education. Different in kind, but illustrative of the definition, is Moreira's evaluation of the Brazilian primary school curriculum from the standpoint of current psychological knowledge (9).

The foregoing general conception of educational psychology was used in selecting publications for inclusion in this review. Two exceptions were necessary in view of space limitations: because the literature dealing with the education of exceptional children is so extensive and the concerns are so heterogeneous, it was felt that this area could not be represented fairly in a brief review; the definition was further delimited to the locus of school problems in order to exclude these studies that, although categorizable under educational psychology, will be reviewed in other sections of this volume. In addition to this criterion of relevancy, reports were examined for their methodological adequacy and their probable general interest value. Since it was not possible to refer to all of the publications meeting the three criteria, those cited in the review should be considered to be samples only.

TEXTBOOKS AND GENERAL TREATMENTS OF EDUCATIONAL PSYCHOLOGY

A special anniversary issue of the *Review of Educational Research* included an analysis of research in educational psychology during the past quarter of a century (10). This analysis, prepared by a committee under chairmanship of Gates, found that such research has been improving but "has been uneven and, on the whole, disappointing." The committee concluded that there is a marked need for better approaches to research design for school-centered research, although educational psychology has been somewhat successful in changing its emphases with changes in the nature of schooling. A three-year review of educationally relevant research in the areas of mental and psychical health was notable because of a liberal inclusion of anthropological materials and methods (11). Several contributors to that review offered psychoanalytically oriented discussions—a fact that may help assuage Watson's regret that the educator has ignored the contributions of psychoanalysis (12).

More than the usual crop of textbooks made an appearance in this period. New collections of readings in educational psychology were edited by Crow & Crow (13), Fullagar, Lewis & Cumbee (14), and Remmers *et al.* (15)—bringing the total to five such collections in three years, with strong rumors that the end is not yet in sight. Stroud (16) and Jordon (17) revised their general textbooks, the latter for the fourth time. The old Kingsley text was revised by Garry (18) in such a manner that it now falls within the category of an educational psychology. A new general textbook was produced by Frandsen (19). A quasi-educational psychology, consisting of an overly brief discussion of educational problems from a psychoanalytic frame of reference was published by Stewart & Workman (20). Students in England

were introduced to a textbook by Peel (21). Rasmussen (22), in Denmark, wrote an educational psychology with emphasis upon the thinking and problem-solving processes, apparently sharing Russell's (23) conviction that this area is of particular current import in education.

The fashionable but poorly defined notion that every teacher should be a guidance worker caught Gordon's (24) attention but left Marzolf (25) unscathed. The former addressed a guidance textbook to teachers; the latter to embryonic specialists. Rogers (26) added another to the rapidly growing catalogue of texts in mental hygiene applied to the elementary school level; the secondary schools continue to go relatively unnoticed by mental hygienists.

Although almost all of the textbooks published during the review period reflect an increased and commendable interest in finding new ways to relate psychology more meaningfully to the everyday activities of the schools, Blair (27) appears to have been most successful, although in the more restricted remedial-diagnostic area and at the cost of sounding somewhat prescriptive. This, however, may be an unavoidable and forgivable risk for any psychologist who has the temerity to undertake the task of offering immediate help to teachers at the level of specific operations.

PUPIL CHARACTERISTICS

Two European studies of developmental processes will probably provide considerable discussion and research activity as soon as translations become available. Kroh (28) and Viethen (29), in apparently independent analyses of recent European research findings, report a present tendency for children to attain physical maturity at earlier ages and a general acceleration in most maturational processes. Although these conclusions are generated from studies designed for other purposes, they are worthy of critical attention and verification studies in view of their import for educational test norms, curriculum materials, and educational organization.

Wattenberg (30) studied 575 runaway boys in Detroit schools, finding that "search for adventure" and "rebellion against parents" were major reasons for such behavior and that school factors were of much lesser import. He further observed that, in most cases, the aftermath of "running away" found parents trying to better their relationship with their sons. Two studies reported in Sweden during the past year are relevant here. Husén (31) noted that children with lesser contact with their mothers had worse school behavior ratings and Marklund (32) obtained a substantial relationship between the degree of disturbance in the school child and both mother-father and parent-teacher attitude differences. A search for patterns in school problem behavior was conducted by Eaton and his colleagues (33), who found that the frequency of problem behavior increased with grade level with an increasing involvement of boys in the higher grades. The differences and trends "observed," however, were not tested for significance, although inspection suggests that they are probably reliable. Weiss (34) re-

ported what may be the first empirical examination of the Korzybskian hypothesis that the " 'is' of identity" in language habits leads to misevaluation. He compared the language habits of delinquent and nondelinquent boys, finding that the former use the " 'is' of identity" to a significantly greater degree than the latter. With this difference noted, we may hope that someone with similar interests will test the hypothesis experimentally, introducing the language habit as an independent variable.

The dimensions of socioeconomic class were extended by several persons, among them, Angelino, Dollins & Mech (35), who identified a positive relationship between the status level of the home and the number of "fears and worries" projected by elementary and secondary school pupils. The hypothesis that social stratification is more important in the formulation of occupational choices than are community, school, and work experience variables was tested and confirmed by Youmans (36). Mitchell (37) factored a comprehensive battery of 18 mental tests administered to 11- and 12-year olds and compared the factor patterns of high and low socioeconomic pupils. He found the same factors essentially present in both status groups, but observed that the organization of factors was much less differentiated for the low status group. Mitchell's conclusion was that this lower differentiation among the "lows" is a partial consequence of increased saturation of the general factor with verbal components; all mental tests become more verbally weighted for low status pupils because of their relative verbal incompetence.

The concept of sociometric status continued to be a popular one in educational research. Miller (38) introduced a measure of "socioempathy" (an individual's awareness of his own and other's status) and applied this measure to superior, typical, and mentally retarded children. His results confirm previous findings of a strong correlation between sociometric status and mental ability and indicate, further, a correlation between mental ability and socioempathy. Bjerstedt (39, 40) reported an extensive study of specificity-generalizability in sociometric choices, in which he found considerable nonspecificity across methods, criteria, and time. Gronlund (41) reports a similar generalization of such choices across the loci of classroom, school, and neighborhood. Such findings are consistent with the conviction that personal preferences of pupils can be used as efficient means for forming intraclassroom groups. Mann (42), who found that generalized friendships can be produced under conditions of part-time segregation of fast learners, may have contributed some relevant data as well, although the lack of appropriate controls makes evaluation of the findings difficult.

Havighurst (43) prepared a comprehensive summary of research relating to the use of the developmental task concept in education, concluding that this concept has fared well in American public schools. Estes (44) tested some of Piaget's conclusions about the development of number concepts and submits some findings that bear upon the educational concept of "readiness for number." Apparently, and contrary to Piaget, (a) if a child could

count at all, he counted correctly whatever the arrangement of objects; (b) children did not confuse the increase in line length with an increase in the number of elements in the line; (c) children did not mistake an apparent increase for a true increase; and (d) if children could project a line in one direction they could project it in another direction.

The expected sex, geographical, and racial differences in school retardation were found by Ypsilantis & Bernert (45) in an analysis of the 1950 census data. Tyler (46) compared the interests of English and American school children, identifying similar sex difference patterns in the two countries and a marked tendency for English children to like fewer things and to dislike more things than American children do. What appears to be the first study of the attitudes of English school children towards the novels commonly assigned in school was reported by Whitehead (47), the results suggesting that the practice of using a single reader should be changed in favor of mixed sets of books. In Scotland, educators were provided with an extensive summary and analysis of educationally relevant and recent research on intelligence (48, 49).

CLASSROOM LEARNING

General.—An interesting and possibly consequential approach to the criterion problem in school-centered research was presented by Cogan (50). Although the criterion of pupil change appears to be the most relevant criterion of teaching effectiveness, this involves instrumental difficulties that will not be reduced in the near future. Cogan suggests that we substitute criterion variables that intervene just prior to pupil change: the amount of required and self-initiated work performed by pupils. Richardson (51), in England, developed a searching analysis of those social-psychological phenomena of greatest relevance to the teacher and spells out some educational hypotheses bearing upon the problems arising because the teacher must communicate on two levels at once—with the individual pupil and with the group of pupils. An excellent illustration of the use of psychology in educational planning and evaluation is contained in the Dottrens (52) report of the Unesco regional seminar, in which representatives of 15 European countries developed plans for the improvement of classroom teaching. Symonds published the third (53) and fourth (54) of his series of reviews of "what education has to learn from psychology"—one on punishment and the other on whole versus part learning; Calvin *et al.* (55) reviewed all the empirical studies of adult learning published since 1930.

The school administrators who have been asking for definitive studies of the effects of nonpromotion on achievement will be interested in the study reported by Caffield & Bloomers (56). The 302 Iowa school systems participating in the Iowa Basic Skills Program were searched for seventh grade pupils who had been failed at least once since the third grade. Some of the 147 pupils so identified were matched in achievement with a promoted classmate, the rest were matched with a pupil in another school system. The

results are striking. Slow learning pupils who repeat a grade and slow learning children who are promoted, ultimately perform at about the same level in the same higher grade. The nonpromotion problem was also considered by Blomqvist (57) in Sweden; he found that the parents of promoted secondary school students differed from those of nonpromoted students in both aspirations for the child and attitudes towards the school.

Reading.—The reading investigations reported during the 12 month period ending in June, 1956 were summarized by Gray (58). Simon (59) analyzed French research relating to the teaching of reading and writing, emphasizing the early influence of Binet and Simon on the reading methods used in French schools. Previous studies indicating the positive influence of kindergarten training on reading progress in the primary grades were confirmed in an investigation conducted by Fast (60), who found that children with kindergarten training achieved significantly higher scores on all reading measures than did comparable children without such training. Three doctoral dissertations on reading, reported by Betts (61), suggest a substantial relationship between literal and critical reading abilities. Webb & Wallon (62) found that, for intelligent college students, more information can be obtained by reading than by auditory presentation, if equal time is available. Tinker (63) investigated the changes that occur in speed of reading and "visibility" of words when the alignment of printed lines departs from horizontal (the line perpendicular to the median plane of the body). He found a significant reduction in visibility and speed, a reduction he attributes to the complicated oculomotor adjustments required by the interfixation eye movements. Several Japanese studies of the learning of *Kanji* (characters) are summarized in a recent issue of *Kiyo* (64).

Many of the available studies of the effect of reading programs at the college level have failed to control motivation and have not adequately randomized subjects in the formation of experimental and control groups. Reed (65) designed a study in which both of these requirements were reasonably met and found that 27 hours of remedial instruction given to an experimental group at Wayne University produced reading rate gains greater than those obtained in the control group, although no differences were found in comprehension, vocabulary, or honor point ratio. The findings held both for the period immediately following instruction and seven months after the termination of instruction. McDonald & Pauk (66) reported that a college remedial reading program based on the study of word derivation and word context led to improved reading speed, comprehension, and general college achievement. The existing conceptions of the nature of reading disability were reviewed by Hansburg (67), who offers the hypothesis that all reading disability cases involve "a motivation disturbance resulting in low frustration tolerance and easy fatigability." Some Flesch was put on the emaciated body of phonics by Ace (68), in England, who compared a visual-phonetic approach to remedial reading with a "mixed method." He reported the visual-phonetic procedure to be slightly but significantly superior against

the criterion of reading age. In view of the criterion used by Ace, it seemed particularly appropriate that the later pages of the same journal carried a plea by Vernon (69) for a distinction between the mechanisms or content of reading behavior and the discrimination-evaluation-understanding objectives in the teaching of reading.

Problem solving and transfer.—Excellent analyses of the educational significance of the relationship between rigidity and problem solving were presented by Carpenter (70, 71). He looked at and evaluated those investigations conducted from the personality approach and from the learning approach, summarizing the educational implications of each and concluding that the learning approach to problem solving has been more productive for education than has the personality approach. Carpenter also reported an experimental study of functional learning versus rote learning of concepts (72), finding that functional learning (seeking underlying principles, manipulating, etc.) was more efficient when the criterion is the retention of and ability to verbalize the meanings of the concepts. A similar design and finding was reported by Forgas & Schwartz (73). Mialaret (74) conducted an interesting variation of the perennial "progressive versus traditional teaching" studies. Even when a "traditional" type of lesson was conducted to the satisfaction of partisan judges, pupils made little progress in ability to transfer what was learned. Calvin *et al.* (75) investigated the interaction of intelligence and social atmosphere with group problem-solving behavior. In a series of three experiments, they found that with high levels of intelligence a "permissive" social climate is superior to a "traditional" climate for group problem solving. Students of average intelligence, however, apparently are handicapped under the "permissive" conditions. There is some indication in Corman's (76) study that more explicit direction is most helpful in problem solving with the more able students while the less able seem to fare equally well with either more directive or less explicit direction. The apparent contradiction between the two studies may be resolved by noting that Corman's dimension was amount of information given, while Calvin's group used the characteristics of the classroom setting. All students of the effects of "permissiveness" and "directiveness" can profit from the caution and results offered by Criag (77). He noted that while experimental evidence favors some degree of learner activity over specification of answers by the teacher, the results have been interpreted by the educator as implying that "the more freedom, the better." His hypothesis was that increased direction of learner activity will effect increases in learning without accompanying losses in retention or transfer. His data are consistent with this hypothesis: the group receiving more direction in discovery of relations learned relations better than those under conditions of independent discovery. Furthermore, with time, the directed group retained more than did the independent group.

Buswell (78), in an extensive and well-designed study, found more variety than similarity in methods used to solve mathematical word problems at both secondary and college levels. This absence of a generalized mode of problem

solving is attributed to a failure on the part of the schools and teacher education programs. Perhaps of even greater interest, are the findings relating to the way in which students responded to questions and problems containing irrelevant data and problems which needed more data for solution. A surprisingly high proportion of secondary school and college students were not able to distinguish between the relevant and the irrelevant. Equally marked was failure of many students to recognize when additional information was needed for solution. Such findings suggest the possibility that the mathematics problems presented in school and in textbooks may too frequently contain all the data required for solution and no data irrelevant to solution—despite the brute fact that everyday existence does not roll over and play dead so conveniently.

Motivation.—Sivertsen (79) leveled a sharp criticism at level of aspiration studies that purport to have educational relevance. He feels that most of these studies involve two "errors." First, there is unnecessary ambiguity in the definition of the concept—he would distinguish between aspiration as "expectation" and aspiration as "intention" (oughtness) since these may not covary. Second, he finds that these studies tend to consist of "the observation of an aggregate of anonymous subjects engaged in contrived and casual tasks that seem to be of little challenge to self-esteem." Illustrations are given of the probably greater value in obtaining observations in a competitive situation where actual social pressures may be ascertained. This viewpoint may have been implemented, in part, by Schroder & Hunt (80), who compared high school boys who avoid failure situations with those who refuse to perceive failure. The "avoiders" tended to set higher goals, use fewer alternative solutions in attempting to solve problems, and performed less effectively after failure. The boys who refused to perceive failure tended to maintain their self-evaluations after criticism, state higher self-evaluations following failure, and overevaluate performance.

In an achievement motivation experiment with college students, Applezweig, Moeller & Burdick (81) found that nonacademic motives had significantly greater effect upon the achievement level of overachievers. Hurley (82) and Johnston (83), pursuing McClelland's findings, reported that verbal learning and arithmetic performance were positively and substantially correlated with achievement imagery. Terrell & Kennedy (84) found that preschool children learn as efficiently as older children under delayed reward conditions, provided that they are able to observe progress towards the goal. The effectiveness of the SRA Self-Scorer was examined by Gilbert (85). He was able to produce greater retest gains through the use of the standard answer sheet accompanied by discussion of results than was possible with the Self-Scorer.

Guidance.—Schmidt & Ruth (86) called for more sophistication in defining guidance roles in education, noting the strong possibility that many current conceptions of guidance operationally mean the rewarding of the blindly conforming pupil and the punishment or nonrewarding of the inquir-

ing and critical pupil. Henry (87), an anthropologist, offers and tests a more complex (and dramatic) hypothesis: the classroom teacher, in her responses to pupils, reinforces the "witch hunt syndrome" (tendencies to docility, competitiveness, confessions, intragroup aggression, and feelings of vulnerability) which children bring to school. Careful and systematic observations of teacher-pupil interaction in several elementary classrooms failed to reveal a general pattern of the kind hypothesized, although isolated instances were identified. Another, and more rigorously defined, set of hypotheses was derived and put to test by Meyer & Thompson (88): (a) boys (since they are more aggressive and nonconforming than girls) receive more disapproval responses from their teachers; (b) girls (since they are more quiescent than boys) receive more approval from teachers; (c) both boys and girls are aware of these differential teacher responses. The data gathered by direct observation in classrooms supported the hypotheses for boys but not for girls.

The importance of personality variables in learning was underlined by the Haigh & Schmidt (89) study. College students were allowed to select either of two sections of a required course, in the knowledge that one section was to be "teacher-centered" and the other "group-centered." At the end of the semester, no significant achievement differences appeared when the two groups were compared. Another extension of the personality variable in education was provided by Hoehn & Saltz (90), who inquired into the degree to which an "average teacher" can improve student achievement by means of systematic individual interviews with students. The results indicate that interviews influenced failure rates and that there is considerable interaction of personality and specific interview technique. The investigators conclude that the average teacher (*i.e.*, one with little sophistication in interviewing) cannot improve achievement by means of student interviews.

Richardson & Surko (91) combined their findings with those of the earlier Glueck studies and conclude that the delinquent's deficiency in school achievement is related not so much to inability to do school work as it is to nonconformity to the standards set by the school. The lowest Wechsler Intelligence Scale for Children subtest scores for this group were those most dependent upon school learning. Horrall (92), in an elaborate study, produced data that suggest that academic underachievement in the case of unusually intelligent college students may be symptomatic of deep-seated personality problems.

Johnson (93) examined the techniques used by elementary school teachers in helping the withdrawn child. It appears that a majority of these teachers deal with the withdrawn child by providing ego-support and "enhancement of self-concepts"—a fact that is interpreted as a favorable commentary on the sophistication of these teachers. Strang (94) asked public school and college students to respond to the question, "What makes studying difficult or easy for you?" The individual differences revealed in the analyses of the responses suggest that it is important for teachers to listen to what children and adolescents say about their study problems. The extensive study of teacher-pupil

interaction reported by Moustakas (95) contains detailed transcriptions of incidents which will be welcomed by teacher-educators looking for illustrations of the phenomena under consideration in the studies reported above.

Using the techniques of the Character Education Inquiry, Canning (96) conducted a six-year study of academic honesty, beginning one year before the establishment of an honor system at Brigham Young University. During the first year studied, 81 per cent of the students "cheated" on examinations; at the end of the first five years of the honor system, this was reduced to 30 per cent. Before the honor system, male students cheated out of proportion to their sex; at the end of the period studied, females were cheating out of proportion to their sex, with the males cheating at a lesser rate.

Audio-visual aids.—Standohar & Smith (97) added another to the list of studies that indicate the superiority of lecture-supplemented films over films alone. More interesting, probably, is the study reported by Craig (98). He notes that available research suggests the possibility that the silent film is more effective than the sound film as a teaching device, if the teacher supplies commentary. He tested this possibility, using sound films versus sound-muted films supplemented by teacher commentary. The secondary school students learned and could retain more from the sound-muted film than from the sound film, whether the teacher commentary was placed before, during, or after the film showing. Parsons (99), using a criterion of amount of information gained, found that instruction by kinescope was more effective than conventional class discussion over a four-month period.

EDUCATIONAL PERSONNEL

Examinations of the relationship between "teaching success" and a wide variety of antecedent conditions continued to appear during the period under review. In most of these studies, the success criterion is too ambiguous to afford usable generalizations. Jones (100), for instance, found that "good" and "average" teachers would be distinguished by a pattern of preservice achievement, temperament, and personality; the nature of the behavior sampled in the criterion measure, however, is not clear. Similarly, Knox (101) found strong relationships between Rorschach performance and a non-reproducible criterion of "teacher efficiency." It appears that more attention is given to the definition of the predictor variables than to the specification of the criterion. Even when the criterion is clear, however, the question of criterion relevancy often remains. Coladarci (102) and Burns (103) have commented on this problem, the latter criticizing the implied conceptual orientation in common procedures for determining the success criterion in educational personnel research.

Rocchio & Kearney (104) found that they could predict the frequency of pupil failure from the Minnesota Teacher Attitude Inventory performance of teachers. Dodge & Clifton (105) produced substantial correlations between a measure of pupil-rapport and (a) preservice peer group rating on social

characteristics; (b) education professors' estimates of teaching promise, made three years before.

Several "Wickman-type" studies were published during this period, as in all previous review periods. They generally confirm the results obtained previously: teachers are not as mental-hygiene oriented as are the specialists. An international flavor is given to this concern by Xydias (106), who compared French teachers' judgments of problem behavior with the judgments of specialists—with the usual result. Stouffer (107) found that elementary teachers are more like the mental hygienists in their views of child behavior than are secondary school teachers, although both teacher groups tended to be more concerned over extroversive behavior than were the specialists. The latter tendency is found in Hunter's (108) results. Brandt & Perkins (109) summarized 16 independent studies conducted during the past 10-year period for the purpose of evaluating the in-service child study programs carried out for teachers by the Institute for Child Study of Maryland and cooperating institutions. Against each and all of several pupil-change and teacher-change criteria, these programs appear to have been almost consistently successful. Oelke (110) examined changes in student-teachers' attitudes toward children during a six-week initial teaching experience. When these subjects were compared to comparable students who had not yet had such experience, they were found to be more accepting of aggression and more optimistic about pupils. Public school teachers were compared with college seniors by Leeds (111), using the Guilford-Zimmerman Temperament Survey, and were found to be more emotionally stable, more restrained, more objective, more cooperative, and more friendly; the college seniors, on the other hand, had more drive and energy and more "social boldness."

Withall (112) pursued an unusually interesting question and used a particularly elaborate technique. He identified a teacher who appeared to be well-committed to the principle of individualizing instruction as much as possible and, using a sound recorder synchronized with time-lapse photography, he time-sampled this teacher's classroom over a 12-week period. His analysis of interactions sampled in this manner showed that this teacher was not distributing her face-to-face interactions with pupils as widely as she assumed to be the case. She was shown the results and agreed to attempt a wider distribution of her responses during the following weeks. The new data indicated that she was unable to produce a marked change in her distribution of time and attention.

The question of whether a school administrator can learn how to behave in a "democratic" manner in the execution of his roles was raised by Hines (113). He found that the extent to which school principals behaved in "democratic" or "authoritarian" modalities could not be predicted from F Scale or GAMIN responses and he concluded that the learning of either of these generalized modes of administrative behavior was not precluded by personality factors.

EDUCATIONAL MEASUREMENT

Measurement and prediction of achievement.—Educators, particularly at elementary school levels, tend to be partial toward the *gain* criterion in evaluating student status and the adequacy of instruction. We continue to be confronted, however, with the problem of making meaningful statements about a gain score with less than perfectly reliable tests. Lord (114) has addressed himself to this difficulty in a paper that is certain to provoke discussion. He proposes a regression formula for estimating true gain from initial scores, final scores, and the extent to which the scatterplot of initial and final scores differs from the plot that would obtain if the two tests had been administered simultaneously. If the inconsistencies in his operations and example are merely typographical (as they probably are), the report is clear enough to permit evaluation and merits the attention of those persons who must or will use gain measures.

Frick & Keener (115) conducted a cross-validation of a regression equation for predicting college freshman grade point average from the Council on Education Psychological Examination and six clinical scales of the Minnesota Multiphasic Personality Inventory (MMPI). The Gough H_r scale, which was designed to predict college achievement, was originally validated against psychology course grades. Bendig & Klugh (116) found that this scale predicts total undergraduate honor point ratio equally well. When two projective tests (Sound Stimuli Test and Picture Stimuli Test) were combined with six commonly used intelligence and achievement measures, Chahbazi (117) found that the multiple correlation with first term college grades was greatly improved over that produced with intelligence and achievement tests only.

The definition of content validity took an interesting turn in the hands of Ebel (118), with some agreement on the part of Lennon (119), and with Huddleston (120) apparently abstaining. Ebel and Lennon suggested that the content validity of a test should not be determined by ascertaining the degree to which it samples a universe of content but by its relevance to instructional objectives, a relevancy best determined by the test user.

The rationale for and the 1952 revisions of the scaling procedures used in the Graduate Record Examinations are presented by Schultz and Angoff (121). Osborne & Sanders (122), noting the tendency for GRE scores to decline with age, asked whether there are factors operating other than age or in combination with age to produce the observed decline. Their study of recency and type of college training among tested students revealed that the rate of decline in scores with age is differential with kinds of curricula, although most of these "curriculum effects" are negligible after age 39.

Two contradicting reports on the value of essay tests were published in England. Penfold (123) found that interexaminer differences and rescored differences were too large for meaningful use. Wiseman (124), however, found that the use of essay evaluations for the same purposes considered by

Penfold was accompanied by adequate reliability and usable validity. Since these two studies are part of a continuing series of investigations, we may hope for some future explanation of how the reported failure to achieve objectivity squares with the reliability and validity claims. Stanley & Beeman (125) compared essay and multiple-choice tests, finding that subject matter groups did not "interact" significantly with the kind of test, suggesting that essay versus multiple-choice should not be thought of as a major issue any longer. A "construction-shift" test of English composition was constructed in multiple-choice form by Thomas (126). Friedman & Fleishman (127) found that the addition of a "don't know" response alternative to "yes-no" and "same-different" alternatives in a sensory discrimination test resulted in increased reliability without loss in validity.

Hewer (128) found that the Minnesota Medical Aptitude Test made fair predictions to the criterion of success in medical school while the correlation between this criterion and either the Strong Vocational Interest Blank or the MMPI proved to be orthogonal. Barthol & Kirk (129), similarly, could not predict success in the first graduate year in Public Health from Strong or MMPI profiles. They did find substantial correlations between the criterion and performance on The Concept Mastery Test and the American Public Health Association Examination.

Intelligence and aptitudes.—Super served as editor of a series of articles on the use of multifactor aptitude tests in guidance (130). Each of the three contributors, Guilford (131), Dvorak (132), and Crowder (133), described and justified the battery with which he is associated. Super's criticism is that the primary concern among the constructors of multifactor tests has been the almost exclusive search for factorial purity, with a minimization of effort to demonstrate validity against an external criterion. Travers (134) extended this criticism to the general area of prediction, noting that present attempts to improve accuracy in the prediction of performance focus more on the characteristics of the measuring instrument than on the dimensions of behavior.

The Davis-Eells Test of General Intelligence continued to be an object of inquiry. Tate & Voss (135) added another study suggesting that the claims made for this test may have been premature. They obtained clear residence, race, and sex differences and the nature of these differences appears to be inconsistent with the "culture fair" pretensions of the instrument. Russell (136) studied native-born, English-speaking, Jewish preschool children and concluded that both the Davis-Eells and the Stanford-Binet underestimate intelligence in the case of Syrian Sephardic Jewish subjects, "for unknown cultural reasons."

French & Worcester (138) tested mentally retarded but physical average children and children in the normal range of mental ability, using the Columbia Mental Maturity Scale and the Stanford-Binet. They found that the CMMS tended to overestimate IQ in the case of the mentally retarded

and recommended that the scale be restandardized if it is to be used for this purpose. On many of the CMMS items more than one valid principle could be educed, although the key provides for only one answer; the suggestion was advanced that such items be excluded or revised since they must attenuate validity. Dressel (139) commented on the general problem of producing multiple-choice items in which only one alternative possesses a unique property and he presents a method for resolving the difficulties involved. When the achievement criterion is the focus of concern, Strand & Bloomers (140) find that the WISC arithmetic, vocabulary, block design, and object assembly subtests produce the best correlations, suggesting the possibility of using only these subtests for the purpose of achievement prediction.

Educators have frequently voiced the notion that an achievement test can be and probably is a learning experience for the examinee. Whitcomb & Travers (141), more specifically, hypothesized that there is transfer of training from item to item in a test and that, contrary to the standard assumption, the difficulty level of an item in an aptitude test is not independent of its position in the test. Their findings confirm their suspicion. There were large transfer effects in the first few item positions, an effect that continued through later item positions, and adjacent items were more highly correlated than nonadjacent items.

Personality, attitudes, and interests.—The large number of reports classifiable under this rubric is indicative of the trends noted by Wrightstone's committee (142). The educator's interest in these variables has continued to increase, although not always critically. The Minnesota Teacher Attitude Inventory was given its usual share of attention during the review period. Sorenson (143) showed that prospective elementary and secondary school teachers could "fake" MTAI responses in the "good" direction and that the extent of such faking is not reduced by anonymity. Eson (144) observed significant changes in the "good" direction over a semester course in educational psychology but concluded that the difference represented gains in ability to recognize the right response rather than any substantial changes in the attitudes of the students. Mitzel, Rabinowitz & Ostreicher (145) identified three response sets in MTAI responses: positive intensity, negative intensity, and evasiveness. Negative intensity tended to increase validity and evasiveness attenuated validity. Apparently, strength of response may be more relevant than scale content in discriminating among teachers. Fishman (146) compared the MTAI test characteristics of teachers in American Jewish Schools in New York City with the MTAI norming group. The nonnorm group resembled the norm group in that teachers of special subjects scored lower than did regular classroom teachers and scores were positively correlated with amount of professional education. The nonnorm group, however, differed from the norm group in that scores were not constant with increased age. Additional data indicated that males scored significantly lower than females and that foreign-born teachers scored lower than American-born teachers.

The fakability of the Gordon Personal Profile was examined by Gordon & Stapleton (147) and Rusmore (148). While differences were observed when the instrument was administered under guidance conditions and in an employment situation, the changes were quite small and appear to justify the conclusion that this instrument is much less fakable than most of the available inventory-type tests.

The analysis conducted by Kingston, George & Ewens (149), based on coefficients of profile similarity between Kuder Preference profiles and occupational preference profiles, failed to reveal differences between students with consistent patterns of vocational interest and those without consistent vocational interests. Gehman (150) asked senior engineering students to fake the Strong Vocational Interest Blank in what he considered to be the most extreme direction for such subjects—the Category V (social) occupations. The resulting profiles were markedly like those produced by those in Category V occupations and quite unlike those found among engineers. A measure of manifest interest was developed by Ewens (151, 152), for use at the secondary school level. The measure purports to identify the manifest interests related to areas used in the Kuder Preference Record. While the reported reliability coefficients are satisfactory, the validity data are not yet promising.

The educationally popular Mooney Problems Checklist was re-examined by Singer & Steffire (153). They found that the differences among sex, age, and interest groups were large enough to warrant more specific and local norms.

Mayo's study of the effect of halo on peer ratings (154) indicated that although halo was influential on such ratings the phenomenon cannot be thought of as a major source of variance in peer ratings. This conclusion is in agreement with that drawn by Suci, Vallance & Glickman (155), who found that peer ratings of current competence are as reliable as ratings of potential competence and that the reliability of neither is affected significantly by the degree to which the person rated is liked by the person rating. Davis & Warnath (156) studied the reliability of the Cunningham Social Distance Scale. With school children, there was high test-retest reliability, by trait, for the degree of acceptance of an individual by the group; lower reliability, by trait, was found for degree of acceptance of the group by the individual. In view of the increasing use of the scale for both purposes, this finding is of topical import.

AN EVALUATION

Frequent note has been taken of the criticism that the activities of educational psychologists tend to be atheoretical. Anyone who examines the more than 400 publications appearing during the review period and classifiable as educational psychology must be constrained to grant considerable validity to this criticism. Despite the many notable exceptions, the manifest concerns are too frequently immediate, unrelated to an identifiable

frame of reference, unclarified with respect to the assumptions involved, and generated more on *ad hoc* grounds than in a theoretical matrix that can suggest more powerful hypotheses and programmatic research of certain relevance to education. A naive and spurious conception of "practicality" appears to obtain in many quarters. The Gates committee (10) has noted this tendency with pardonable alarm and, in one of the more delightful metaphors in recent professional criticism, suggests that "this practical emphasis kept the work down to earth but in some ways grounded it." That there is continuing improvement in this state of affairs, as a goodly proportion of the work cited here may suggest, is a cause for some optimism. However, if the APA Education and Training Board is correct in predicting that, as a consequence of current popular criticism of education, educators will increasingly look to psychology (156), the presumed mediators of this "look" will have to develop more theoretical sophistication and interest than are now generally characteristic of them. Stephens' recent analysis of the phenomenon of "schooling" (158) is an exemplary illustration of the kind of thinking required.

Another, and probably not independent, limitation in many of the studies examined is found in the poor designs and erroneous operations performed. Stanley's plea for better and more appropriate techniques and designs in educational research (159) is a timely one. The fact that it finds an echo in Japan (160) suggests that remediation efforts may appear even closer to the center of research activity in educational psychology.

The best of current educational psychology, a sample of which is cited here, indicates clearly that the models and precedents for vigorous, rigorous, and relevant research in educational psychology do exist and continue to multiply in number. In view of this, and because optimism is always the better part of criticism, the reviewer ventures the secular prayer that educational psychology is really a great field coming out of a bad period.

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STATISTICAL METHODS¹

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INTRODUCTION

An attempt was made to present a comprehensive survey of the literature pertinent to quantitative methodology in psychology. However, the increasing number of relevant papers necessitated some kind of compromise. A balance had to be maintained between extensive coverage and critical appraisal of the present status of selected subjects—with the author's personal interests and biases affecting this balance.

The status of statistical methods is brought out in a mythical tour through the statistical universe, conducted by Gertrude Cox (28) in her presidential address before the American Statistical Association. Three major continents are visited: (a) descriptive methods, (b) design of experiments and investigations, and (c) analysis and theory. In each of these, the fascinating features of the principal countries are pointed out, and new frontiers can be seen almost everywhere. Although this is not a bibliographical paper, it will give the reader an excellent idea of the state of progress of statistical theory and suggest areas for fruitful research.

By way of general reference to the statistical literature during the period of review, it might be noted that a number of excellent textbooks have appeared. They all make a sincere effort to present the material in a modern fashion to meet the clamoring challenges for mathematics and statistics to be written and taught in accordance with our evolving technological progress. At the elementary level, two such books are Sprowls (120) and Cramér (29), with the former being in the class of "nonmathematical" statistics texts and the latter requiring a modest mathematical background.

Another elementary text, employing a minimum of mathematics, which is exceptionally lucid and truly new in approach is the one by Wallis & Roberts (131). It is intended for the nonspecialist in statistics but provides real insight into the nature of statistical work mostly in the fields of the social sciences, business, and economics. An attempt by Adams (2) to develop "basic mathematico-logical concepts of statistics" at an elementary level probably will not meet the author's expectations. It might well turn out to be a useful text for a second course in statistics.

¹ The survey of the literature pertaining to this review was completed in May, 1957.

² The writer wishes to thank the several members of the professional staff of System Development Corporation who made critical appraisals of specific articles, and especially Margot von Mendelssohn, who is largely responsible for the very comprehensive bibliographical treatment.

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An interesting monograph by Meehl (86) reviews studies comparing mechanical and nonmechanical methods of combining data for predictive purposes. He sees the distinctive contribution of the clinician to be the creation of the hypotheses that relate and apply general psychological principles to the individual case, while the determination of the regularity with which these hypotheses are supported by later events is the concern of the statistician.

In the long history of attempts to define some of the fundamental ideas in the theory of statistics and its application, the recent book by Savage (112) is a valuable addition. Of special interest to psychologists should be his postulates expressing the consistency and rationality of human behavior. The book might best be described as a contribution to the theory of scientific reasoning, with emphasis on statistical methods.

The growing recognition of the value of interdisciplinary work is especially evident in the field of statistics. No longer are research studies easily compartmentalized into neat packages. This was immediately evident to the reviewer when he attempted to organize the literature. In many instances the several sections set forth are largely the result of a need to keep the material within some manageable bounds rather than of any intrinsic distinctions. Some new peripheral fields are reviewed in the next two sections and then somewhat more conventional groupings of material follow in the remainder of the Chapter.

HIGH-SPEED COMPUTERS AND NUMERICAL ANALYSIS

There has been a continuing growth in the planning, development, and application of high-speed electronic digital computers. One aspect of this activity which will be of special interest to psychologists is the increasing provision being made by computing centers to encourage researchers to use their facilities. While a certain amount of computational work has been accomplished by research psychologists on high-speed digital computers, it was largely through the endeavors of specific individuals. During the past year scientists in universities actually have been invited to find new uses for existing machines, and plans have been laid to introduce machines in new areas. One striking example is the announcement by the Oak Ridge Institute of Nuclear Studies that it will make available to universities throughout the region the services of the ORACLE, its high-speed electronic digital computer. Most of the colleges in the New England area are invited to use an IBM 704, without cost, at the new Massachusetts Institute of Technology computation center. Still another example of this trend to increase the universities' awareness of the capabilities of modern high-speed computers and to assist them in their research programs is the proposed Western Data Processing Center now being established at the University of California at Los Angeles. These facilities, which will include an IBM 705 by the Spring of 1958, are planned for the use of business researchers and students and faculty from colleges and universities in eleven Western states.

To take advantage of the new opportunities becoming available for carrying out extensive computations, one would have to gain some knowledge about the machines themselves as well as information about the types of problems adaptable to electronic computers. The lack of adequate material, suitably written, about the data-processing industry (the field of high-speed electronic computers) is pointed out forcefully by Forrester (43). Actually, quite a number of books, reports, and articles have appeared on the subject. However, most of them are not written for the research worker who would like to learn how to exploit such new machinery but who is not a computer engineer.

For the initiate seeking a systematic introduction to computer arithmetic and the logical basis for computer programming, Richards' (100) book probably is one of the best. The text introduces the student to Boolean algebra and then systematically proceeds to addition, subtraction, multiplication, and division in both the binary and decimal modes. To integrate the arithmetical operation meaningfully into the total computer system, the author describes the organization of the IBM 701 computer and sketches basic programming procedures in its use. A more general introduction is given by Canning (24) of some of the complexities which arise in planning for and implementing an electronic data processing system. A good survey of the digital computing field is presented by Wilkes (137). This book covers the history of automatic computers, the electronic circuitry of computers, and the philosophy of coding.

Faster, Faster, a monograph by Eckert & Jones (39), explains in non-technical language how an electronic calculator operates, the nature of the problems it solves, and the form in which the problems must be presented. The exposition is really a description of one of the fastest calculators now in operation—the IBM Naval Ordnance Research Calculator (NORC). Nonetheless, it is one of the best references for the reader who wishes a broad perspective on the nature and capabilities of the modern high-speed electronic computers.

Psychologists, in general, have not made much use of high-speed computers, but there are some striking exceptions. In a study of speed factors Lord (82) used the Whirlwind I computer at M.I.T. to factor analyze a 33-variable matrix of correlations by Lawley's Maximum Likelihood Method. He discusses programming aspects for the computer as well as indicating the iterative process employed. His use of computers also took him to the AGO factor matrix rotator (an analogue type electronic computer). Wrigley and his associates have made extensive use of two very similar high-speed computers, the Ordvac at Aberdeen Proving Ground and the Illiac at the University of Illinois. An example of the use of the Illiac in the determination of principal axes factor solution is given by Wrigley & Neuhaus (143). Additional references to the use of high-speed computers to solve problems in factor analysis appear in a later section.

The rapid development and successful application of large-scale digital

computers in the last decade has provided special impetus to work in that branch of mathematics known as "numerical analysis." Several books and a good many articles have appeared on this subject in the past few years. Perhaps one of the most basic of these is Householder's *Principles of Numerical Analysis* (62) which appeared in 1953. Of the books published during the period under review, Nielsen's *Methods in Numerical Analysis* (93) is intended as an elementary textbook, being directed toward students in engineering. Its emphasis is on methods primarily useful with desk calculators, but these methods can also be adapted to use on high-speed digital computers. The coverage of interpolation methods is good, but the treatment of systems of linear equations in Dwyer (37) will be found much more satisfactory.

Hildebrand (59) treats the subject in a slightly more advanced manner. His book is clearly written, and the many problems are well selected and add materially to the value of the text. At a much more advanced mathematical level is *Numerical Analysis* (33), containing the papers presented at the Sixth Symposium in Applied Mathematics of the American Mathematical Society. The fact that such a symposium was held, and that it was so successful, is a clear indication of the recognition of the importance of mathematical research in the theory and art of computing.

The reader interested in numerical analysis would do well to refer to the rather complete bibliographies appearing in Householder's text referred to above and to his more recent article (63), containing 321 references, which brings the original bibliography up to date.

OPERATIONS RESEARCH AND RELATED TOPICS

During the past decade considerable progress has been made in the development of new theory and methodology in areas which are peripheral or borderline to the more conventional statistical field. The theory of games of strategy, linear programming, and operations research are cases in point. An excellent article by Thomas & Deemer (121) suggests the outstanding properties of gaming and nongaming techniques as approaches to competitive situations. In the course of scientific investigation of such conflict situations, it may be expeditious to study a simpler situation that may be formulated as a game, viz., a set of rules that govern and fully determine the possible courses of conflict. They point out that the two principal techniques employed in the study of a game are analytic game theory and operational gaming, and they set themselves the problem of clarifying what the latter technique can do. The similarities in spirit and method of simulation, Monte Carlo, and operational gaming are noted, while the refinement in meaning of these concepts is made quite distinct.

The research psychologist interested in exploring another area of rapid growth in recent years will find Rohde's (103) bibliography on linear programming invaluable. While discussions of linear programming go back to the 1930's, it is only since the end of World War II that the mathematics

has been developed extensively, and the bibliography lists 266 such items. In the 1940's work was begun by psychologists and statisticians on the personnel classification problem—a subject closely related to linear programming.

A classical problem in linear programming is the "Hitchcock-Koopmans transportation problem," for which Ford & Fulkerson (42) provide a simple description of a new computing procedure. This problem is in a class of linear programming problems that may be described as follows: Determine the $m \times n$ numbers in the matrix (x_{ij}) , $i = 1, 2, \dots, m$ and $j = 1, 2, \dots, n$, such that

$$\sum_{ij} c_{ij}x_{ij} = \text{minimum},$$

subject to the constraints

$$\sum_j x_{ij} = a_i, \quad \sum_i x_{ij} = b_j, \quad \text{and} \quad x_{ij} \geq 0,$$

where a_i , b_j , and c_{ij} are given nonnegative integers, and $\sum a_i = \sum b_j$. The proposed computational procedure appears to be more efficient than the better known simplex method.

An intuitive approach is used by Ferguson & Dantzig (41) in describing the application of linear programming to a problem of allocation under uncertain demand (as contrasted with fixed demand). Since the theoretical basis for the method was presented in an earlier paper by one of the authors, the present paper provides the reader with a simple example of how the linear programming procedure is applied under uncertainty.

Some relatively new journals which have not been tapped in previous *Annual Reviews* may be of interest to the reader concerned with quantitative methodology. One of the foremost of these is *Operations Research*, published bimonthly by the Operations Research Society of America. An excellent introductory paper by Ackoff (1) discusses operations research as a science rather than a profession. He outlines the major phases of an operations research project as follows: (a) formulating the problem; (b) constructing a mathematical model to represent the system under study; (c) deriving a solution from the model; (d) testing the model and the solution derived from it; (e) establishing controls over the solution; and (f) putting the solution to work. Before discussing each of these phases in some detail, Ackoff makes an observation about the development of operations research in the U.S.A. In the early days of "operations research" (1941-46) the work was done primarily by mathematicians and physical scientists. Since the end of World War II, and especially since 1950, an increasing number of statisticians, industrial engineers, economists, behavioral scientists, logicians, and computers (human and electronic) have moved into operations research, and this new blood has contributed greatly to its development. The paper contains a very comprehensive reference list (153 items). Other papers in *Operations Research*, utilizing techniques which may be of interest to psychologists, include the following: Bishop's (14) description of a simplified method for

scheduling production to minimize costs of overtime operations and inventory storage as a function of sales requirements; Morse's (88) observation that the principal use of statistics in operations research is in the design of experiments and in the preparation of data for analysis, while probability theory is the more important tool for the construction of a mathematical model; and Laurent's (77) statistical treatment of some of the problems arising in bombing situations.

Churchman, Ackoff & Arnoff (26), in collaboration with 11 others, have prepared a very comprehensive treatment of operations research. Their book emphasizes the scientific approach to management problems, tracing the evolution of operations research from its start in a military context to its widespread application in business, industry, and civil government. No advanced training in mathematics is required for comprehension of the chapters on description of operations research, formulation of the problem, general concept of the model, and administration of operations research. However, a higher degree of mathematical sophistication is required for an understanding of some of the more complex mathematical models useful in operations research.

A new journal, *Management Science*, is published quarterly by the Institute of Management Science. Indicative of the type of material which may be of interest to the industrial or research psychologist is the paper by Salvesson (107). Here a management statistician attacks the problem of maximum production of a product when fixed numbers of two other products must be produced on a single production line. A solution is reached by setting up a specific mathematical model, and, in essence, employing a small-scale linear programming technique.

Another journal devoted to quantitative method which may be relatively unfamiliar to the research psychologist is the *Journal of the Association for Computing Machinery*. A very brief history of the origin of the Association for Computing Machinery, including the scope of the organization and the development of its Journal, is presented in the Presidential Address (64).

PSYCHOMETRIC SCALING AND TEST THEORY

In the more conventional areas of psychometric scaling, selection and combination of items, reliability and validity of tests, the literature continues to expand each year. Among the interesting papers are two concerned with the paired-comparisons method, one with the successive intervals method, a couple with absolute judgments, and several which are devoted to other aspects of scaling.

Gulliksen (50) presents a method for scaling by paired comparisons when dealing with a matrix of incomplete data. A least squares solution is developed in matrix notation and an iterative computational procedure provided. The procedure should extend the usefulness of the paired-comparison technique, since the number of judgments required for large numbers of alternatives can be reduced to a workable level. In another paper, Gulliksen (51)

uses an extension of psychophysical scaling methods to develop four laws that relate subjective value of a commodity to amount of commodity. The four laws are: an additive law (linear), a square root law, a logarithmic law, and a negative exponential law. The food preference experiment employed in the previous paper is again used here to test these value laws. Ninety-two subjects were asked to state their preferences for five component stimuli (each an ordinary serving of a meat such as beef) and 10 composite stimuli, each consisting of two of the components. Scale values of components and composite stimuli are determined by the paired-comparisons method. Pearson's Method of False Position (briefly reviewed by Gulliksen) is used in fitting the laws to the data. Gulliksen provides a clearly stated procedure for investigating the laws governing preferences or value judgments when no simple physical measurement of the commodity can be made and when the usual scaling methods do not give a zero point.

A significant contribution to psychophysical scaling methods is made by Rozeboom & Jones (105). The scaling method of successive intervals is evaluated in terms of the degree to which computations from experimental data can be expected to differ from theoretical true values as determined from the definition of the base scale. Errors in base scale values are shown to be of three types: (α) errors due to unequal variances of the distributions used to compute interval widths; (β) errors due to nonnormality of the distributions; (γ) sampling errors due to the estimation of cumulative proportions of the interval boundaries from finite samples of the measuring distributions. No one type of error is found to be large but the β -error is the most difficult to evaluate. With respect to total error the authors state that if the sample sizes of the distributions are sufficiently large then "the extent to which interval ratios computed by the method of successive intervals diverge from the corresponding theoretically 'true' values should not exceed 10 per cent of the latter . . ." After considering some basic problems in psychophysical measurement, Rozeboom & Jones conclude that until analyses similar to theirs are performed for other psychophysical scaling methods, the method of successive intervals should be accepted as the basic standard against which other techniques should be validated.

A technique for scaling of stimuli without establishment of arbitrary units of measurement is proposed by Rosner (104). Using the method of absolute judgments he considers the results of M responses R_k ($k=1,2,\dots,M$) to N stimuli S_i ($i=1,2,\dots,N$) arranged in a matrix of conditional probabilities $p_i(k)$. Each row of this matrix corresponds to a stimulus, and he suggests as a measure of similarity between stimuli, the cosine of the angle between any two row vectors, viz.,

$$\cos \phi_{ij} = \frac{\sum p_i p_j}{\sqrt{\sum p_i^2 \sum p_j^2}},$$

all sums being over k . By use of this function, mean scale separations between stimuli are obtained and hence scale values are determined for the

stimuli. Salzinger (108) considers the attempt to modify (i.e., shift) the frame of reference on an observer's behavior in the method of absolute judgment. He reviews nine distinct techniques of treating judgment data, pointing out the conditions under which each of the methods is most appropriate.

A "higher-ordered metric" scale of preference is developed by Siegel (118), making use of the theory of games and lattice theory. He clearly defines an *ordinal* scale of utility, and gives the conditions it must satisfy to be *consistent* and *transitive*. Then he sets forth the properties of a *higher-ordered metric* scale, and gives an empirical example to show how such a scale is derived and what its relationship is to an ordered metric scale. Rimoldi (101) proposes the "hypothesis of linearity" (corresponding to Gulliksen's "additive law") in an attempt to determine the relationship between scale values for a single stimulus and the scale values of combinations of these stimuli in pairs.

Webster (133) proposes a coarse grouping transformation of test scores into five categories (9 per cent, 19 per cent, 44 per cent, 19 per cent, and 9 per cent) and then shows how the usual test statistics can be obtained using the transformed values. Formulas are given for the item-test point biserial, an estimate of the variance of the original test scores, the correlation of the total test scores with the criterion, an item selection standard, and a reliability coefficient. Applications of the formulas to an example demonstrate that the transformation is fairly efficient and precise enough for practical purposes.

While a variety of methods of pattern and profile (as distinguished from additive) scoring has been in vogue for a decade or longer, not very much has been done toward the development of a mathematical foundation for these methods. An important contribution in this regard is made by Lubin & Osburn (83). For the case of t dichotomous items and a quantitative criterion, they employ a least squares approach to show that the *configural scale* (a t th degree polynomial of the t item scores) produces maximum validity. The configural scale, by analogy with Meehl's scoring method, is defined as follows: a mean criterion score is computed for each of the 2^t possible answer patterns, and this score is assigned to each individual with the given answer pattern. In addition to developing several theorems which establish the maximum-validity property of the configural scale, Lubin & Osburn indicate how the analysis of variance technique can be used for tests of significance to answer questions on the linearity of the relationship for maximum validity, on the effect on the configural scale validity of adding items to the test, on the difference in validity of the total score and configural scale, etc. The authors recommend that an approximate least squares solution (with a reduced number of terms) be computed first, and only if it does not provide an adequate fit to go on to an exact least squares solution. They also indicate the possible usefulness of the configural technique for item analysis. However, they caution against indiscriminate preference for configural scoring, stating:

In general, any procedure involving configural scoring implies a very small number of items and a large number of subjects. Also, unless the items have been specially constructed, or there are good theoretical grounds for believing that non-linear relations exist, the usual total score will probably give maximum validity.

McQuitty (85) classifies persons according to their predominant patterns of responses to the individual items of a test by a method which he designates *agreement analysis*. The agreement-disagreement dichotomy underlying the technique of agreement scoring distinguishes it from methods of configuration analysis.

There have not been too many papers on the popular problem of reliability of psychological tests. Several studies do relate to this question and to the closely associated question of validity bounds for given reliability. Also, a number of papers are concerned with maximizing validity by appropriate item selection.

Towner (124) seeks to determine the effects on the obtained reliability coefficients of failure to meet in a practical situation all of the theoretical conditions underlying the particular technique used. For an empirical example, reliability coefficients computed by the odd-even split-test with Spearman-Brown correction, the Kuder-Richardson Formulas "20" and "21," and the Guttman "L₁" and "L₂" techniques, all yielded approximately the same result, although none of the methods completely met the basic underlying assumptions. He concludes that ease of computation rather than any theoretical considerations should dictate the method of determining the reliability of a test. White & Saltz (135) give a critical appraisal of the various indices that have been proposed for measuring a test's reproducibility. A note by Angoff (6) suggests a method for estimating the nonspurious correlation of a part of a test with the total test. Formulas are derived by Woodbury & Lord (139) for weighting tests in a battery and assigning administration time so as to maximize reliability of the weighted composite.

The problem of selecting a subset of items from a large pool so as to produce a test which will correlate maximally with an external criterion has been under investigation for many years. A procedure is presented by Webster (132) for discarding k items from an experimental test so that the resulting test will have a higher validity than the original one. It is shown that the validity of a reduced test will be greater than that of the original when the following inequality is satisfied:

$$\frac{C_{Tc}^2}{V_T} > \frac{\sum C_{jc}(2C_{Tc} - \sum C_{jc})}{2C_{kT} - V_k}, \quad 2C_{kT} > V_k,$$

where C stands for covariance, V for variance, T is the total test, c is the criterion, j is a test item, and the sum is on the index j over the k items to be rejected from T . In order to make practical application of these relationships in item selection, the author suggests that only items be considered for inclusion that have appreciable variances and validities.

McCornack (84) is critical of psychologists who are unduly impressed with high correlations. He points out that as long as the correlation between two different keys (e.g., weighted and unweighted scores) is not 1.00, it is possible that the validities may differ to a statistically significant and practically important degree. A table showing the maximum and minimum validities that can occur for one key, given the validity of the other, is presented for correlations ranging from .900 to .998 between the two keys.

Humphreys (65) refutes the contention that an attenuation paradox in test theory exists, namely, the (supposed) lack of monotonic relationship between reliability and validity. He demonstrates that if no assumption is made concerning the shape of the criterion distribution, the relationship between reliability and validity is monotonic. The assumption of normality of the criterion distribution on the other hand, does attenuate item validities with the error becoming progressively larger with higher validities. He deplores the use of the assumption of the continuous normal distribution in test theory and test statistics instead of accepting test data in the form of rank-order point distributions.

Recognizing that Wishart's generalized distribution of the covariance matrix for a set of correlated variables does not provide a practical solution, Brogden (20) develops an expression for the sampling errors of validities of a set of correlated items when the total number of items is large. Among the various assumptions underlying the use of the approximation formula is that all items have equal difficulty, hence it is recommended that the approximation be applied to pools or subpools of items reasonably homogeneous with respect to difficulty. Also assuming equal item difficulty, Keats (73) proposes a formula for estimating error variance at a particular score level. His result, being independent of the reliability coefficient, indicates that the error variance at a particular score level remains constant although the reliability coefficient varies from population to population for the same test.

A brief review of research in educational measurements from 1930 to 1955 is presented by Wrightstone and others (140). As stated by the authors they attempted to cover important but selected contributions of research in the various areas of testing and in statistical methods related to test construction. Their review includes 187 references.

CORRELATION, REGRESSION, AND PREDICTION

The appropriateness of various measures of correlation is still a subject of considerable interest. During the past year it has not been the specially devised coefficients of psychometrics, but rather the more conventional statistical measures that have come up for discussion. The Pearsonian coefficient of correlation has often been used to approximate the extent of exact correspondence of paired observations, although its more appropriate usage is in those cases where the concept of regression is involved. Situations where this is particularly true include the measurement of test reliability by test-

retest or comparison of alternate items; comparison of an observed set of values with a set of theoretically deduced values; and situations where the degree of "likeness" of paired groups (such as attitudes of husbands vs. attitudes of wives) is to be determined. Robinson (102) feels that the intraclass correlation coefficient described by Fisher would be more appropriate in these cases. He further proposes a generalization of the intraclass coefficient, called the coefficient of agreement, which permits the use of more than two samplings of responses to be considered simultaneously in estimating the extent of agreement.

Shortcuts and machine techniques have been proposed to lighten the burden of computation in connection with multiple regression and curve fitting. A simple graphic method for fitting a straight line, when the x -values are equally spaced (e.g., time-series data), is presented by Askovitz (7). An economical method for rapid computation of higher moments by use of standard IBM accounting machines is presented by Jaspen (67). For the reader having access to an IBM 704 electronic computer, a program (66) is available to perform a multiple linear regression and correlation analysis from an input of raw data.

While the method of path coefficients dates back to 1921—to the first published work of Sewall Wright—it was not until much later that it gained any real popularity even among geneticists. Its usefulness is more generally appreciated now as a special type of multivariate analysis. Li (81) presents an excellent account of the concept of path coefficients in general, and points out many examples of its impact on population genetics. He develops a fundamental theorem in the theory of path coefficients:

$$r_{XY} = \sum_i p_{X_i} p_{Y_i} + \sum_{ij} p_{X_i} p_{Y_j} r_{ij}, \quad i \neq j$$

where it is assumed that the variables X and Y are linear combinations of "common causes," Z_1, Z_2, \dots, Z_n and uncorrelated error variables E_1 and E_2 , expressed in standardized form as:

$$x = \sum_i b_i z_i + k_1 e_1 \quad \text{and} \quad y = \sum_i c_i z_i + k_2 e_2,$$

and $r_{ij} = r_{Z_i Z_j}$. Then the path coefficients from the common causes to the two variables X and Y are $p_{X_i} = b_i$ and $p_{Y_i} = c_i$. The correlation between two variables is thus seen to be the sum of all the paths connecting them. Li refers to a more comprehensive mathematical treatment of the subject in *Statistics and Mathematics in Biology* (75), Chapter 2 by Wright and Chapter 3 by Tukey, and to his own more elementary exposition (80).

Theoretical developments in prediction and classification have been somewhat sporadic in recent years, except for the intensive work on the personnel classification problem during the past decade. Essentially, this problem consists of determining the optimal assignment of N individuals to k jobs when there are quotas for each position, and the contribution c_{ij} to the general effort which any individual i would make when placed in job j is assumed

to be known. While this problem is closely related to linear programming, work on the former by psychologists-statisticians has been relatively independent of work on the latter by economists-statisticians.

Dwyer (38), who has made many contributions to the personnel classification problem, presents an extension of some of his earlier work which is especially effective for dealing with a small number of positions. The new "detailed method of optimal regions" also provides objective procedures for determining the successive increments v_i to be subtracted from the contributions c_{ij} , making the method amenable to machine operation. Hence, the procedure should be useful in handling more complex personnel classification problems.

A procedure for redistributing the specified over-all testing time for a battery of tests in such a way as to obtain optimal prediction of multiple criteria is presented by Horst & MacEwan (61). While an exact solution was possible for the special case of a single criterion, the method developed for multiple criteria involves an iterative solution. Lev (79) considers the problem of computing subtest weights so as to maximize the prediction of a criterion with the restriction that the weights be nonnegative. By means of an iterative computational procedure, involving repeated trials of increments in regression weights, each trial produces an increase in the multiple correlation until a maximum is reached.

Saunders (111) notes that situations in which predictability varies as a function of a discretely structured variable may be treated statistically by analysis of covariance. On the other hand, he proposes treating situations in which continuous variables moderate predictive powers of other variables by a moderated multiple regression method. The equation

$$y = \bar{y} + \sum_i a_i x_i$$

used in ordinary linear regression is modified by substitution for a_i a linear function of a second group of predictors z_j which are supposed to moderate the influence of the original predictors. The new equation can be put in the form

$$y = k + \sum_{ij} c_{ij} x_i z_j,$$

under suitable change of scale. This equation can be used to construct a geometric surface which permits ready visual comprehension of the regression.

FACTOR ANALYSIS

Many articles on factor analysis have appeared during the past year, ranging from routine reports of practical applications to valuable contributions to the theory. Among the most important problems in this field are the determination of communality or when to stop factoring, the objective determination of simple structure, and statistical inference in factor analysis. Significant advances have been made in all of these areas.

A concerted attack on the communality problem was made by Wrigley,

his associates, and a number of consultants, with the support of a U. S. Air Force contract at the University of California, and the results appeared as a mimeographed series of Contract Reports (53, 54, 55, 68, 69, 126, 141, 142). In the first of these reports, Wrigley (141) employs an empirical approach to demonstrate the relative efficacy of some 15 different methods of estimating communality, and to study the rate of convergence when an iterative procedure is used with each of these methods. Wrigley concludes that Burt's method (the highest correlation, with an increase for variables with high correlations and a decrease for variables with low correlations) is the most effective for use with a desk calculator, and that the squared multiple correlation of each variable with all the others is the best method for estimating communality if a high-speed electronic computer is available.

An excellent review of the principal mathematical work on the problem of estimating communalities is presented by Kaiser (69). He organizes the conditions that must be imposed in order to arrive at a unique set of communalities for a given correlation matrix, under three schools of thought: the minimum rank approach, the statistical approach, and the psychometric approach. He concludes that the minimum rank formulation is inadequate, that the statistical approach adds an essential missing ingredient (a statistical test for the minimum number of factors), but that the psychometric approach really provides the most acceptable answer to the problem. Guttman (55) and Tryon (126) formulate the communality problem as the determination of the squared multiple correlations in the universe of tests, with no special emphasis on a small number of factors. The literature contains many expressions (including those of Rao and Hotelling) of misgiving about and uncritical acceptance of the notion of a small number of common factors. A detailed summary of the arguments against small rank in the population is given by Wrigley (142).

In another report, Kaiser (68) develops a direct procedure for the determination of communalities, without factoring the correlation matrix and without prior assumption regarding the number of common factors. The procedure consists of successive approximations to the squared multiple correlation of a given variable on the common parts of the remaining variables. Apparently there is no assurance that the process will converge for an empirical correlation matrix. Guttman (54) has developed some theorems on improved bounds for communalities which might find useful applications in Kaiser's iterative procedure.

A relationship between communality and multiple correlation is the subject under consideration in another report by Guttman (53). Here he provides a simple proof of the long-established relationship:

$$R_j^2 \leq h_j^2,$$

where R_j is the multiple correlation of variable j on the $n-1$ remaining variables, and h_j^2 is the communality of the same variable. The importance of this inequality for the problem of estimating communalities is shown by

Guttman (52) in another paper. He classifies the various approaches that have been used to estimate communalities under four categories: (a) trial-and-error exact formulas, (b) exact formulas for special cases of the correlations matrix, (c) successive approximations, and (d) lower bounds. Although biased in general by being underestimates, he argues that the lower bounds approach provides "best possible" estimates of communalities for an arbitrary correlation matrix; and that no general lower bound to h^2 has yet been established other than R^2_j . He further establishes that, under certain conditions (when the number of common factors is small in relation to the number of variables), the lower bound must actually equal communality in the limit as the number of observed variables increases.

Assuming the communality problem solved, Guttman (56) proves that formulas previously known to provide sufficient conditions for factoring a matrix are also necessary. Thus he establishes the fact that all possible factoring methods (whether one or many factors are extracted in one operation, and whether or not the correlation or score matrix is factored) can differ only in the choice of weight matrices. The formulation of the factoring problem in such a compact form has a certain heuristic value as well as providing a simplified approach to practical computing procedures.

An excellent account of statistical inference in factor analysis is presented by Anderson & Rubin (5). The treatment, in mathematical statistics terms, first considers the existence of a solution of the form:

$$\Psi = \Lambda \Lambda' + \Sigma,$$

i.e., what conditions Ψ must satisfy in order that this equation can be solved. Next, in answer to the question of whether there exists a unique solution, the necessary restrictions that must be placed on Λ and Σ are pointed out. The problem of determining Λ and Σ , given Ψ , is shown to be related to the methods of estimating parameters. In considering the hypothesis that the model fits, a maximum likelihood test is suggested, but the authors admit that there exists no adequate sampling theory to determine when to stop extracting factors.

Lawley (78) calls attention to a paper by Bartlett (10), summarizing various approximate χ^2 tests, and sets himself the problem of extending certain of these which involve the latent roots of sample covariance and correlation matrices. A case in point arises in factor analysis where the effects of the k largest latent roots have been removed and the hypothesis is made that the remaining roots are equal. Assuming that the first k of the latent roots of the true correlation matrix (involving p variates) are distinct, the problem is to test the hypothesis that the residual $p-k$ latent roots are equal, although unknown. The criterion used for testing this hypothesis does not follow a χ^2 distribution, although it will approximately do so if the k roots are large and the residual roots small.

A mathematical development of an iterative procedure for the calculation of the eigenvalues and eigenvectors of a real symmetric matrix is pre-

sented by von Holdt (129), while a computer program for the JOHNNIAC has been prepared by Golub (47). Of course, the principal factors are obtained from the eigenvectors simply by the normalizing process of dividing by the square root of the sum of their squares and multiplying by the square root of the corresponding eigenvalue.

Some excellent work has been done in recent years on objective methods for the rotation of axes to obtain simple structure. Typically, such methods lead to oblique simple structure. Concerned with the difficulties that may arise in the interpretation of oblique factors, Schmid & Leiman (115) propose a method for transforming such a solution into an orthogonal one, still preserving simple structure, but usually involving a larger number of orthogonal factors. The hierarchy of higher-order oblique factors is made explicit in their model of an orthogonal hierarchical factor solution. Computing techniques for the determination of simple structure are offered by Neuhaus (89, 90), who develops two computer programs for use on the National Cash Register Computer (CRC 102-A). One of these is for the quartimax method, proposed by Neuhaus & Wrigley (91) several years ago. This method determines an orthogonal transformation which maximizes the variance of the factor contributions, i.e., the squared factor loadings. The second computer program is for the determination of an objective simple structure as set forth by Tucker (127). His method of rotation to simple structure involves the isolation of linear constellations with dimensionality one less than the common-factor space.

Harris (57) considers the relationship between factors obtained from a correlation matrix with unities in the principal diagonal and factors from a matrix with communalities. First assuming that the elements of these matrices are population values, he obtains the relationships:

$$F_c = F_u T \quad \text{and} \quad T = S_u S'_c$$

where T is the transformation matrix, and F_c , F_u and S_c , S_u are the factor matrices and factor score matrices based upon communalities and unities, respectively. Then, when the correlations are regarded as observed values, he shows that the same relationship satisfies Lawley's requirement for a maximum likelihood solution when T is defined by a principal-axis factoring of another matrix based upon F_u . He also shows that the same solution is a first approximation to Rao's canonical factors, but points out that the computation involved in Rao's method is less laborious than trying to obtain F_c by way of F_u .

Gibson (45) calls attention to the formal identity of Cattell's proportional profiles in factor analysis to Lazarsfeld's latent structure model, stating the common problem in the form: "Given two Gramian matrices, R_1 and R_2 , of order n and rank m , to find corresponding orthogonal factor matrices, V_1 and V_2 , that are proportional by columns and are known to exist from theoretical considerations." He adapts a convenient solution for the latent structure equation to proportional profiles, eliminating in this instance both

the communality and rotational problems. However, this method assumes the number of factors to be known at the start. Gibson notes that small changes, random increments, and rounding errors seem to produce a sizable shift in the position of the reference frame before the maximum degree of column proportionality is obtained, thus casting some doubt on the utility of proportional profiles or latent structure solutions.

The application of factorial and stochastic techniques as a method of describing certain learning processes is considered by Burt & Foley (22). The effects of each presentation of a series of tests or stimuli lead to a data matrix, expressed as a linear operator on an hypothetical factor matrix. The psychological interpretation is that if there exists any significant difference between one data matrix and the next, then learning has occurred, presumably because the factor matrix has changed from one trial to the next. The linear operator which transforms one data matrix into another essentially describes the process of learning and by further simplifying assumptions this learning matrix may be factored into its latent roots.

Several studies were directed toward the development of theories of intelligence based upon factor analytical results. Burt & Howard (23) demonstrate that the distributions and correlations obtained from measuring human intelligence conform fairly closely with the correlations deducible from the multifactorial theory of inheritance (i.e., that differences in intelligence are determined by a large number of genes, segregating according to Mendelian principles and each producing effects that are small, similar, and cumulative). Then on the basis of this theory, they analyze the variance observed in measurements of intelligence and estimate the proportions due to hereditary and to nonhereditary factors. Ahmavaara's monograph (4) is concerned mostly with his conceptualization of the mind and with the expression of his theoretical position regarding factor analysis, but the most interesting and probably most significant feature is the formulation and systematic application of his mathematical method of comparing factor studies.

An attempt is made by Guilford, Kettner & Christensen (48) to trace the history of a single factor (usually designated "General Reasoning") through its many appearances and interpretations. According to Baggeley & Cattell (8), the apparent lack of evidence for psychological meaningfulness and stability of factors has kept psychologists from using factor scores for individuals. They discuss the effort involved and the appropriateness of exact and approximate procedures for estimating oblique factor scores.

Applications of factor analysis to many fields other than intelligence are just as prevalent as in past years. Some of these include: Guilford & Zimmerman's (49) study of temperament; a critical review by Borgatta, Cottrell & Meyer (17) of three empirical studies concerned with group behavior; and a Q-analysis, of 16 primitive tribes for 2500 items, conducted by Schuessler & Driver (116).

The forecast in the Review of three years ago, "factor analysis may be

expected to graduate from a technique suspected by many to one respected by most," continues to hold good in the light of more recent activity in this field. Considerable progress continues to be made toward the clarification of the many difficult problems that have been inherent in factor analysis.

ANALYSIS OF VARIANCE AND RELATED TOPICS

The organization of statistical topics dealing with design of experiments, estimation, and inference is especially difficult. In this section an arbitrary grouping is made of such topics as experimental design, particularly in analysis of variance, multivariate analysis of variance, analysis of covariance, variance components, and statistical inference. Developments in the mathematical theory for these areas are progressing at a rapid pace, and only such work deemed to be relevant to psychological research is covered here.

More evidence of the continued interest of mathematical statisticians in formulating mathematical models for analysis of variance appears in a paper by Scheffé (114), in which several models are discussed at length, and methods of analysis are compared and contrasted. Two papers, both concerned with average of mean squares in factorial experiments, are typical of the attention being paid to the logical basis of analysis of variance, and both indicate that the field is a treacherous one in which there are no simple and safe rules for the experimenter. In the first, Cornfield & Tukey (27) consider a pigeonhole model which includes the linear model as a special case, and develop formulas for average values of mean squares for 2-way and 3-way classifications. In the second paper, Wilk & Kempthorne (136) explore linear models of completely randomized factorial experiments with a view to establishing a logical basis therefor. Results are given for three factors, both for proportionate and unequal numbers in the subclasses. Neither of these papers is easy reading, although the difficulty is not with mathematics, but rather with the fundamental question of appropriate assumptions and their effect on the proper mode of analysis.

With the wide application of analysis of variance designs, situations frequently arise where the model is not completely specified. In such cases, preliminary tests of significance have been used to determine if error variance components may be pooled. The advantage of this pooling procedure is to increase the number of degrees of freedom and thereby increase the sensitivity of the F -test. Bozovich, Bancroft & Hartley (18) critically examine the consequences of such pooling procedures.

Charts of the power of the F -test are presented by Fox (44). No new information is involved, but tables previously published have been used to obtain an arrangement designed to answer the following question: What degrees of freedom of numerator and denominator of F are required to obtain a specified power β against a specified alternative?

Certain symmetric functions, called polykays, are used by Tukey (128) to derive formulas for the variances of variance components in balanced designs where the purpose of the analysis is to estimate the size of the con-

tributions to the over-all variance from the corresponding sources. The derivation requires only the assumption of independence, and does not require infinite populations or normality.

In situations where there are several estimates of the variance of normal populations, there are established tests for the equality of the variances, but these do not distinguish which of the variances are different. David (34) considers two approaches for ranking variances of normal populations: (a) an adaptation of Tukey's gap-test which effects a separation of the variances into two groups whenever the successive mean squares exceed a critical tabulated value; (b) a more elaborate multiple decision procedure, based upon the distribution of the maximum F -ratio, which gives an ordering of the variances into overlapping classes.

Ray (99) provides tables to expedite the application of sequential tests to the general linear hypothesis. The common analysis of variance situations to which this is applicable include (a) one-way classification by groups, and (b) randomized blocks. In the process of constructing these tables, Ray develops a number of approximations to the confluent hypergeometric function. His tables also suggest the expected sample sizes of the sequential processes.

There are many psychological studies in which data are available from large numbers of subjects on many variables, but analysis of variance is not used because of the computational work involved. Electronic computer methods described by Hartley (58) make possible repeated analyses (as used in Q -technique studies) on all the data, rather than on just a few representative parts. However, the limitations of storage space in such computers and the time necessary to write a computer program or the alternative of standardized design patterns, restrict the extent to which machines can be efficiently utilized.

While the general theory of comparison and design of experiments has received considerable attention, only a few particular problems have been investigated thus far. Bradt & Karlin (19) study some relatively simple dichotomous situations and find that the optimal design is exceedingly complicated. The authors treat the problem of decision between two hypotheses where there are two types of experiments available and where a sequential rule is desired to select the next type of experiment at each stage.

In the area of sampling theory, Kish (76) has developed a method of constructing confidence intervals for clustered samples which avoids some of the difficulties in other methods—approximations, analogies, rephrasing of the problem, or simply refusal to make probability statements. His method is based on subsamples which have the same type of clustering as the whole sample, and an unbiased estimate of the variance is thereby obtained.

Bartholomew (9) proposes a method for testing whether a sequence of events is occurring at random in time or space when the alternative is a trend. Typical situations in which this problem arises involve industrial accidents or machine failures. He employs the theory of sequential analysis

to derive the test and demonstrates its use with coal mining accident data. Noether (94) also presents sequential tests of randomness against the alternative of linear trend and cyclical trend which are claimed to require a smaller expected number of observations than other tests in current use.

Billewicz (13) proposes a sequential test of the difference between proportions when matched pairs are used. While the usual formulas for the sequential test for significance of a difference between proportions assume no correlation in response between members of the same pair, Billewicz' formulas provide for such correlation.

In the case of the Poisson distribution, Rao & Chakravarti (98) present tables for exact tests of goodness of fit, homogeneity, and deviation of the frequency of zero. The tables are accompanied by a discussion of the defects of the common large sample tests where the Poisson parameter is small, and recommendations are made about the choice of the proper test.

Barton & David (11) describe a possible model for the distortion of the random sequence for use as an alternative hypothesis to the hypothesis of randomness, and they illustrate this model with Spearman's ρ . For any sample of n pairs (x_i, y_i) which are arranged in rank order, the probability $p_0(R)$ is known under the null hypothesis, namely, that in the bivariate population generating this sample the correlation between x and y is zero. If there were a correlation between x and y , then there would tend to be more matched pairs than might be expected under the null hypothesis. Barton & David propose for the alternative hypothesis that there has been an increase in the number of matchings and calculate the power of Spearman's ρ to detect a positive correlation in the bivariate population.

The question of what to do with an apparently faulty or "aberrant" observation in an otherwise valid set of data is considered by Bliss, Cochran & Tukey (15). If it is defective and the experimenter accepts it (or if it is sound and he rejects it), his results will be biased. To minimize the risk of making a wrong decision, the authors propose a statistic T , which is merely the largest range divided by the sum of all the ranges. The probability distribution of T is approximated and tabled. If an observed ratio T exceeds the probability level $P = .05$ the set represented by this largest range is assumed to contain an aberrant observation, which is identified by inspection and rejected.

NONPARAMETRIC INFERENCE

A field of increasing popularity with research psychologists involves nonparametric methods. Two decades ago rank order methods were essentially limited to the median, mean difference, and Spearman's ρ . There were statisticians, however, with sufficient vision to predict the development of an entire statistical science based upon rankings corresponding to the existing theory based upon the metric scale. Ten years elapsed before Kendall's book appeared devoted exclusively to rank correlation methods. In recent years, more and more papers have been appearing on nonparametric meth-

ods, and finally the first full-length text by Siegel (117). In a very lucid fashion, Siegel presents "distribution-free" or "ranking" techniques of hypothesis testing for one, two, and k (related or independent) samples, as well as a discussion of the appropriateness of the various tests. He also presents several "ranking" measures of the degree of correlation between variables in a sample and tests of significance of their observed association.

A mathematical basis for the general ranking problem is proposed by Blumen (16). He suggests the usefulness of probability processes as the point of departure for measuring the concordance of rankings. Thus, instead of viewing a particular ranking as a random selection out of some hypothetical universe, he considers that some process (e.g., Markov) is producing the possible rankings, and that each ranking has its probability determined by the character of the process.

The growing awareness of the usefulness of nonparametric correlation coefficients in psychological research is exhibited in several papers devoted to Kendall's τ :

$$\tau = \frac{2(P - Q)}{n(n - 1)},$$

where P is the total number of agreements in ranks of the two series, Q the number of inversions, and n the number of cases in the sample. Schaeffer & Levitt (113) point out instances where this coefficient has advantages over Pearson's r and Spearman's ρ ; and give methods for its computation, with and without ties, as well as methods for determining partial rank correlation and tests of significance. Cartwright (25) presents an efficient method for the computation of τ designed especially for large samples and multiple ties. In a note concerned with "arbitrary ties" in rankings, Adler (3) develops a modified version of Kendall's τ , namely:

$$\tau = \frac{P - Q}{P + Q}.$$

The number and frequency of tied rankings are regarded as arbitrary when such ties result from the ranking process itself rather than from the qualities of the individual being ranked.

The natural next step in the psychological use of τ is taken by Cureton (32), who develops the following "rank-biserial" correlation between one variable represented by rankings and another variable characterized by a dichotomy:

$$r_{RB} = \frac{P - Q}{N_1 N_2 - t_1 t_2},$$

where P and Q are defined as above, N_1 and N_2 are the respective numbers in the two categories, and t_1 and t_2 are the numbers of bracket ties in the high and low categories respectively. Cureton shows the equivalence of his formula to both τ and ρ .

Torgerson (123) considers the question of correlation between two variables when one or both of them consist of ranks within subsets of the total sample. The nonparametric test for correlation which he proposes is an ex-

tension of Kendall's test of association between two rank orders, and he argues by induction from two empirical cases that the normal distribution provides an adequate approximation to the probability.

Sarhan & Greenberg provide tables for making linear estimates of location and scale parameters of exponential distributions (110) and of the normal distribution (109) from censored samples of size up to 10. (Censored samples are those in which the r_1 smallest and the r_2 largest observations are missing.) The estimators are unbiased and have minimum variance among all unbiased linear estimators. The tables include the case $r_1 = r_2 = 0$, and hence may be used for ordinary uncensored samples. This method should prove especially useful in making estimates of standard deviations from many small samples, since the property of linearity obviates the need for squares and square roots present in the traditional method.

Kamat (70) considers a test for the equivalence of two distribution functions (viz., that the two samples came from the same population) using a criterion based upon the range of ranks in the two samples. The exact probability distribution of the proposed test statistic is developed and percentage points are computed and tabled for a combined number of observations in the two samples not exceeding 20. The calculation of percentage points from the exact distribution becomes impracticable when the sequence becomes large, and so Kamat obtains the first three moments and other characteristics of the distribution of the test statistic in order to derive good approximations by which to extend his table.

Among the many papers devoted to nonparametric statistics, two deal with the power of rank order tests in the two-sample problem, where the alternatives are one-parameter distributions. The null hypothesis is that both samples are from the same population, while the alternative is typically that the two distributions differ in a location parameter. Hodges & Lehmann (60) compare the two-sample Wilcoxon test with the t -test by means of asymptotic relative efficiency as defined by Pitman. Dwass (36) considers a class of rank order tests, and the locally best test is characterized.

A preliminary step in the evolution of a nonmetric factor analysis of the kind proposed by Thurstone indicates two ordinal consequences of the factor analysis model, as pointed out by Bennett (12) in his development of a generalized form of the "Unfolding Method" of Coombs. First it is shown that the total number of different ways in which s subjects can be ranked by linear functions of d factors is $R(s, d) = R(s-1, d) + (s-1)R(s-1, d-1)$, a number which becomes very large even for moderate s and d . Secondly, the constraint of the number of rankings due to dimensionality is generalized as follows: in a simplex formed by a vector-set S , an interior vector X cannot be separated from the set S by any ranking generated by a linear function of the coordinate axes.

STATISTICAL DECISION THEORY

A major break-through in mathematical statistics came with the introduction of the theory of statistical decisions by Wald (130) just prior to his

death in 1950. The interest in decision theory has been increasing in recent years as noted in the two previous Reviews. An attempt has been made here to cull from the literature those works which have some immediate psychometric implication. One of the first of these efforts was stimulated by an eight-week summer conference on "The Design of Experiments in Decision Processes" a few years ago. A book by Thrall, Coombs & Davis (122) presents the 19 papers of this conference on statistical decision theory, game theory, learning theory, and measurement theory. The report includes experimental studies as well as the mathematical theory.

Just published are two monographs emphasizing the experimental approach to decision making. Davidson, Suppes & Siegel (35) offer a theory for the prediction of human decision making in situations involving uncertainty, and present experiments for the purpose of testing the adequacy of that theory. These experiments led to the development of a psychometric technique for interval measurement of utility and unique measurement of subjective probability. Another important contribution is an experimental test of a linear programming model for measuring the cardinal utility of non-monetary outcomes. The other monograph, by Cronbach & Gleser (30), applies some of the recent developments in decision and utility theory to personnel decisions. Essentially, an endeavor is made to extend the theory of tests to a precise statement of the parameters involved in their use as well as their construction. The inadequacy of measurement theory in applied science is stressed as a basis for offering decision theory as an alternative model. Although the authors do not discard measurement theory, they do specify where it applies rigorously and where it may serve as an approximation to the ideal solution, and add the important parameters involved in decision—payoff function and selection ratio—which are necessary to determine the value of the testing procedure to the decision maker.

Simon (119) points out that learning theory and game theory which aim to provide theory of rational behavior, are closely related to statistical decision theory. He shows that in a partial reinforcement experiment the asymptotic behavior of the subject can be derived from the assumption that the subject is behaving rationally in a game-theoretic sense and is attempting to minimax his regret.

Edwards (40) criticizes conventional statistical procedures involving the acceptance or rejection of the null hypothesis or answering the question "is the alternative hypothesis true?" He maintains that what is required in practical situations are answers to questions such as "Should Method B be used instead of Method A?" or "Should a test which has demonstrated validity for measuring a rare trait be used in a selection program?" The answers to these questions should be in terms of the relative costs and relative payoffs of alternate procedures. The theory of one-tailed tests is explored in two papers by Karlin & Rubin (71, 72). The mathematical proofs are given in the latter, while the former contains more of an explanation. The theory is shown to be a special case of Wald's decision theory, and one-tailed

tests are appropriate to distributions with a monotone likelihood ratio. Many of the classical testing problems satisfy this criterion, so that intuitive procedures are justified in terms of decision theory. These papers are valuable in filling the void between the general theory and specific applications.

A short paper by Keeping (74) combines an expository treatment of statistical decision theory with the necessary mathematics as he presents several examples in the field of quality control, where the practical need for decisions becomes so obvious.

MISCELLANEOUS TOPICS IN STATISTICAL INFERENCE

Many articles and short notes on computing aids, tables, and approximation formulas have appeared during the past year. In this section a representative group of such papers is discussed, with some stressing probability theory, others sampling, and still others tests of significance.

Noting that probability values for χ^2 are not available for really high χ^2 , Wishart (138) derives an expression for the chance of χ^2 being less than, or at most equal to, its degrees of freedom ν . He develops two formulas which may be very useful for tabulating χ^2/ν probabilities to considerable accuracy; or which may be used in abbreviated form to obtain approximate probabilities without the use of tables.

The determination of exact probabilities for 2×2 and $2 \times r$ contingency tables is considered by Sakoda & Cohen (106), making use of binomial coefficients instead of factorials. Tables for the binomial coefficients are given for n through 60. Also, a set of inequalities is provided for calculation of cumulative probabilities for a given contingency table (useful for a one-tailed test). Two convenient nomographs for determining the significance of differences in frequencies when these are arranged in 2×2 contingency tables are proposed by Trites (125) and Bross & Kasten (21).

For a finite population of N elements of which number M possess a favorable characteristic, the probability of drawing a random sample of n elements (without replacement) of which exactly m possess the favorable characteristic is given by the hypergeometric function:

$$P_{nm}^{NM} = \frac{\binom{M}{m} \binom{N-M}{n-m}}{\binom{N}{n}}.$$

Nicholson (92) derives a new normal approximation to a sum of such hypergeometric terms over an interval $\lambda \leq m \leq \nu$, which is a direct generalization of Feller's normal approximation to the binomial distribution. The results of this extension provide upper and lower bounds on the hypergeometric sum and thus give an indication of the relative error inherent in the approximation.

A comprehensive numerical investigation of the comparative accuracy of various approximations to the cumulative binomial distribution is made

by Raff (97). Tables of probabilities over rectangles for the correlated bivariate normal distribution are presented by Owen (96). Along with the tables are given a summary of formulas useful for finding volumes over rectangles and a special two-dimensional interpolation scheme. Gilbert (46) provides tables for the exact distributions of a number of matches for decks containing up to 18 cards. He also considers approximate distributions for use with larger decks and provides a mathematical model to compute significance levels for matching experiments. Mitra (87) presents tables for tolerance limits for a normal population based on sample mean and range or mean range which are especially useful in quality control. Weiss (134) shows how typical reliability growth curves might be fitted to measurements of the time to failure of a complex system in which the failure occurs according to a Poisson-type distribution.

Two papers are concerned with confidence intervals for a proportion. Crow (31) presents confidence intervals for the true proportion π of individuals with a given characteristic in an infinite population when r individuals have the given characteristic in a random sample of size n . Confidence limits are given for the confidence coefficients .90, .95, and .99 for all cases where $n \leq 30$. Noether (95) considers the problem of comparing the success rate of an experimental method with that of a standard method, giving rise to the confidence interval for the difference. Because the difference in proportions is subject to an arbitrary upper bound, an alternative "effectiveness index" is recommended as measuring the actual difference between the two rates in terms of the maximum possible difference.

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INDUSTRIAL PSYCHOLOGY¹

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Consumer decision making, consumer buying behavior, the savings process, and occupational success and failure, have generally been studied, with little cross reference, by independently acting groups of economists, sociologists, statisticians, and psychologists. The time is ripe for a change and the year 1956 can, if we will but take advantage of it, mark the beginnings of a most important and significant series of imbrications.

To support this statement let us begin our review with a research project which, while of the utmost importance to the industrial psychologist, will have been little noticed by him. This is the three-volume series on the savings process conceived and edited by Goldsmith (92). Coming into fruition late in 1955 and early 1956 there is, admittedly, in the 2246 pages comprising the series much that will not interest the average industrial psychologist. We would, therefore, direct his attention only to certain sections in Volume III. There, among many other things, he will find a savings formula the correlates of which, if subjected to further study, will provide much of the imbrication of which we speak.

It cannot help but be obvious, when appropriate thought is given to the matter, that many of the factors suggested as, and in fact found to be, significant correlates of the savings process (defined by Goldsmith as change in earned net worth over a period of time) are the same as those found to be most useful in predicting occupational success and, in some cases, buying behavior. We refer, of course, to items such as net worth, income level, amount of life insurance owned, minimum current living expenses, indebtedness, number of dependents, and age. For some of this year's evidence see Brumberg (39), Elliott (68), Glick & Miller (89), Robbins & Beckman (183), Scollay (192), Sharp & Mott (196), and Soar (202).

If saving and dissaving, and occupational success and failure can to some extent be explained in terms of the same background factors, here is the basic raw material which some psychological Einstein ought to be able to wrap together in the same package. In this package he will, of necessity, incorporate into his general formula the age-growth concept (long a basic ingredient in all Binet-type mental testing) which Peterson (78, 178) has successfully utilized in developing the predictive value of various background and personal history items for vocational success and failure. In other words, he will evaluate the significance of any intended correlate of occupational success, saving or dissaving, and buying behavior, with reference to the norms for an appropriate age group. This he will do because the vast ma-

¹ The survey of the literature pertaining to this review was completed in April, 1957.

jority of variables found to have predictive value, in each of the areas of which we speak, are those which, many times, can be expressed as a function of age and, possibly, of net worth.

By designing two scales: (a) an economic maturity scale consisting of a composite of background items from which the influence of age has been removed and (b) an interest maturity scale consisting of a composite of background items from which the influence of net worth has been removed, our psychological Einstein would find himself in possession of two most significant variables with which he could begin to explain much that we now know, but do not completely understand, as to why the same background predictor variables turn up independently in so many different types of study. If industrial psychologists can do no more than just begin to see some of the implications growing out of this line of reasoning, the year 1956 will easily be viewed, some 10 or 20 years hence, as one which paved the way for a type of understanding which, heretofore, has not been available.

Within the framework of the thoughts just suggested we can, perhaps, pick up certain ideas from Von Mises (221) and from Shackle (195) both of whom urge that between economics and psychology there is a vast no-man's land crying out to be explored. Not so much, however, from the Freudian approach of Weisskopf whose "Psychology of Economics" (231) turns out to be nothing more than an attempted psychoanalysis of various economists; nor from Hickman & Kuhn's non-Freudian, yet theoretical, attempt (112) to show how, in terms of a "self-theory," an individual derives his plans of action from the roles he plays and the status he occupies in each of various groups.

Those interested in "digging deep" will certainly wish to familiarize themselves with Katona & Mueller's review (130) of the results of three years of interviewing relative to consumer expectations; with Cole's study (55) of "Consumer behavior and motivation"; with Friend's study (76) of the contrasts in spending patterns between home-owners and nonhome-owners, between older and younger customers, and between manual and nonmanual workers; with the study edited by Clark (53) of "Consumer Behavior"; with Brady & Kerschbaum's closely related study (34) showing that one's habits do not—even for income reverses—quickly change; with Sharp & Mott's study (196) of husband-wife decision making; and with Graham's (96), Hoyt's (117), and Marsh & Coleman's (157) group behavior, cultural, or Warnerian type studies of the extent to which consumer buying behavior is related to, determined or affected by, social class and conservatism.

There are many who will, of course, claim that really to understand buying behavior one must delve into one's unconscious and that the best if indeed not the only way to do this, in spite of many protests, is via the analytic depth interview technique. And so between the Motivation Researchers of the analytic school and those of statistical persuasion the battle still rages (4).

Dichter (60, 61), who claims that those who package beer (8) should

consider its personality, says that in order to understand the consumer one must

think in terms of family satisfaction . . . give the customer "moral permission" for spending and enjoyment . . . utilize the fact that the customer's mind is plastic . . . offer him . . . psychological partnership . . . follow the changes in the public taste . . . be aware of the changed role of the sexes [This, I missed!] . . . foster personal relationships . . . help him . . . plan . . . and satisfy his desire for individuality. . . .

In contrast to this slapdash mixture, Graustein (97) says, quite properly, that the only use of motivation research of the type advocated by Dichter is that of furnishing a few ideas to be tested in traditional fashion. It is obvious, continues Graustein, that in contrast with the kind of analytic method advocated by Dichter, one can get more significant and much more practical information from " . . . thorough well-designed questions, properly cross analyzed," particularly, we might add, if done in the manner described by Hyman (120) or by Boyd & Westfall (33).

Not so, says Black (29), for there are two kinds of "why's" to which we want answers. First, we want to know how "personality needs or emotional impulses," conscious or unconscious, verbalized or not, some of which Leonhard (147) in a completely uncritical volume claims might be discovered by means of various projective techniques, determine or affect customers' preferences and buying habits. And how a "given service or the image of a given brand subtly satisfies or fails to satisfy man's wants and needs": e.g., security, definitions of which, as a result of a further study of the much-studied General Motors, "My Job Contest Essays," Thompson & Davis (213) provide. To find these things out is the purpose, says Black, of motivation research of the analytic variety. In this regard Black is morally if not in fact explicitly supported by Wells (233). But we also want to know, continues Black, the conditions under which motives will and will not function and this, says he, is where the statistician comes in.

Or where, say Burck (41) and Stryker (210), both writing for *Fortune*, the two camps should, in the light of reasons given by McKittrick (160) and after the examples set by Herzog of McCann-Ericson and Wroe Alderson of Alderson & Sessions, join their forces. In doing so they might well consider seriously, says Burck, not only Herzog's combined use of nose-counting, depth interviewing, structured questionnaires, and pretested advertisements, but also an extension of the Alderson consumer clinic idea which after a series of depth interviews with various housewives requires a "homogeneous" group thereof to attend a clinic and there "play games" designed to duplicate their behavior in the actual market place. And by so doing to demonstrate, at least to Alderson's way of thinking, the invalidity of the Vicary authored "impulse" theory of buying behavior (220).

During the year at least seven methodologies having to do with consumer reaction came in for some kind of attention. Sandage (189) described a research panel that has been in successful operation for seven years, thereby suggesting that consumer panels need not wear out. But we had better con-

sider in this connection Parker & Hackman's finding (173) that generalized attitudes toward the army do, in fact, affect "the acceptance accorded QM items of issue." Merton, Fisk & Kendall (165) revised their well-known manual on "the focused interview," a type of interview used when it is known by the researcher that the interviewee has been involved in a particular situation. Zimmer (241) attempted, with partial success, to predict non-response bias from a "response-nonresponse probability dimension"; and Bogart (32) waxed enthusiastic about applying, in a field traditionally dominated, said he, by statistical research, challengingly new but, to this reviewer, rather ill-defined methods of qualitative research. Vicary (220), meanwhile, advocating a wide extension of the use of the word-association technique, a technique which he most incorrectly categorizes as one of "the best validated psychometric methods . . . now . . . in use . . .," reported that by its application he had found that the spring season was, at least for women, psychologically the longest in the year.

Benson (26) and Benson & Platten (27) describe a successive interval method of measuring preferences; Mindak (167) describes a way of applying, as a means of measuring the effectiveness of advertising, Osgood's idea of the "semantic differential"; and, finally, Greenberg (98) describes a projective approach in which the interviewer, by design, gets each interviewee to engage on a role playing basis in friendly argument. Such for example as that used by King & Janis (135) in comparing "the effectiveness of improvised and non-improvised arguments in creating opinion change," and comparable to that used by Lieberman (148) who demonstrated that attitudes are affected by persons taking on, in an official capacity, the bonafide roles of foreman and steward.

Human relations research or, rather, the subject of "human relations" has become almost a fetish [See Katz (131) and McNair (162)]. It is now almost 30 years since the rightly famous Hawthorne studies, and it would seem that ample time had been given for even the most obtuse to get the idea that the matter of human relations is important. In this reviewer's opinion, as well as in that of Spates (203), the sale has been made. See, for example, Maier & Danielson's "evaluation" (154) of the judicial and human relations approaches to problems of discipline, and contrast their results with those reported 25 years ago by LaDame, in her book on "The Filene Store." Perhaps Goode (93), Hershey (111), McConkey (158), Unterberger (218), and Wright (238), not to mention the rest of us, can stop our exhorting.

Most research in this area continues to be predicated upon two "nice" but sometimes questionable assumptions: first, that happy workers have high morale and second, that workers with high morale are those who, in terms of more production, better earn their keep (36, 150). Few and far between, nowadays, are studies which suggest that morale or happiness or job satisfaction are worthy ends in and of themselves. The passion of the day is to prove that high morale does, in fact, increase productivity.

In view of the foregoing comment, a direction in which morale researchers might well consider "trending" is one which would cause them to consider

job satisfaction or morale indices as, in Saunders terminology (190), moderator variables. And, as such, as factors which might differentially affect the relation between a dependent variable like production and an independent variable like a selection test; or, the relation between accident rate and work load or as in Castle's study (49) absence; or, as in Shepherd & Walker's study (198), absence and shift; or, as in Berkowitz's study (28), the relation between effectiveness in combat and crew-liking and perceived and actual crew attitudes; or, as in Cleven & Fiedler's study (54), the relation between "tap-to-tap time" and interpersonal perception on the part of foremen; or, as would be suggested by Jarrard's review (121) of the literature relevant to "empathy," equivalent terms for which seem to be projection, perceptual insight, or perceived attitude, the relation between empathy and selected dependent variables. In other words let us place less stress upon morale as an independent variable in its own right, and view it, rather, as being indicative of an atmosphere in which certain other relations can, as may prove desirable, be maximized or minimized. The rather large number of negative or inconsequential results being secured in accord with the traditional approach which involves relating morale directly to production would seem to suggest that if the subject of human relations is as important as many would have us believe—and it assuredly is—some new degree of creativity must be injected into the field.

Specific activities which, during the year under review, resulted in publication were, first, the development of new or, at least additional, measuring devices: the Life Insurance Agency Management Association's "Inventory of Job Attitudes" (149); Aalto's "scale measuring attitude toward working for the government" (1); Burwen, Campbell & Kidd's sentence completion test (44) for "measuring attitudes toward superiors and subordinates"; Renck's SRA Employee Inventory which, as between Wherry (234) and Baehr (7) has, because of alternate methods of factor analysis applied thereto, led to dispute; Mahoney's Guttman-type "unidimensional scales for the measurement of morale" (153); Hemphill's "Group Dimensions Descriptions Questionnaire" (109); and Glickman's test of naval knowledge (90).

Second, the discovery or disclosure, by Dunnette, Uphoff & Aylward (65), and by Ferber (70), that a substantial proportion of those who in response to questionnaire inquiries indicate "undecided" are in fact ignorant of the subjects in question.

Third, the evaluation and "blessing," by outsiders, of certain industrial relations work in general, e.g., Fitzgerald's report (73) of how 67 British productivity teams, set up by the Anglo-American Council on Productivity, and representing both management and labor, viewed with admiration, apparently, much that was good about American production; and the return of the compliment, by Ned Hay (107) who, while visiting England found, in five British companies, a satisfactorily "high degree of development of employee relations practices" coupled with a "statesmanlike view of responsibilities and sound ideas about corporate organization."

Fourth, new descriptions of administrative procedures designed to

facilitate the flow of communications: Ray's story (182), for example, of Indiana University's "open door" policy for its nonprofessional staff; Keown's monograph (134) on factors which affect company and, even more difficult, union downwards communication; Higham's article on "basic psychological factors in communication" (113); Crane & Hoffman's "how-to-do-it" book (56) on the "Successful Handling of Labor Grievances"; and Doohar's United States collection (64) and Chisholm's even more comprehensive English collection (51) of various articles on "how to communicate" in industry.

Fifth, case studies such as Banks' study (13) in the British steel industry of "the attitudes of wives" toward "continuous shift work," work which meant that their husbands frequently worked on weekends; Gilman's insightful study (87), in the cotton textile industry, of "Human Relations in the Industrial Southeast," a study of the "social processes . . . involved in the phenomenon of acculturation"; Williams & Mee's interesting "Cases and Problems in Personnel and Industrial Relations" (235); Case's "Personnel Policy in a Public Agency" (48), the largest full scale example of how successfully to use democracy in industry; Zaleznik's "Case Study of Work and Social Behavior in a Factory Group" (240); Lombard's analytical study (151) of ecological and organizational factors which influence behavior and social interactions among department store salesgirls; Paterson's imaginative experiment, in the RAF, in the management of men (174); Blau's "study of interpersonal, i.e., extra-organizational, relations in two government agencies" (31); Burling, Lentz & Wilson's study of "The Give and Take in Hospitals" (42), supplemented, nicely, we might say, by Mishler & Tropp's study (168) leading them to suggest that we be rather cautious in "applying inferences derived from the level of social structure to the level of individual motivations"; Barnett's (14) and Juran's attempts (129) to anthropologize, i.e., to apply anthropological terminology to line and staff; Kornhauser, Mayer & Sheppard's study (144) of auto workers; Rosen & Rosen's study (185) of union members' attitudes toward their union, and Gibbs' reflections (85) "The Worker and His Tools."

Sixth, more general treatises, such as Heller's report on "New Developments in Industrial Leadership" in England, America, West Germany, and France (108); Shartle's chatty summary (197) of the Ohio State University leadership studies which have included reports by Campbell (46), Scott (193), Stogdill *et al.* (206, 207, 208); Bendix's "Work and Authority in Industry" (20); Bursk's "Human Relations for Management" (43); Walker's "Industrial Relations in Australia" (223); Knox's "Introduction to Industrial Sociology" (140); Calhoun & Kirkpatrick's "Influencing Employee Behavior" (45); Laird & Laird's "New Psychology for Leadership" (145, 146) and Kellogg's article (133) on "the top-flight supervisor," popular and, for this reason, uncritical summaries of research in group dynamics and human relations; Klein's summary of experimental studies (138), and Hare, Borgatta & Bales' book of historical, theoretical, and experimental readings

on "small groups" (103); Golden's summary (91) of some 13 studies, many of which had been published prior to 1956, on the "Causes of Industrial Peace under Collective Bargaining"; and Pigors & Myers' revised "Personnel Administration" (179), and Northcott's (171), and Knowles' (139) thought-provoking texts on "Personnel Management."

This last book stresses history, theory, and policy, and makes a plea to the effect that we give up the mechanistic ideas which make most of us nothing more than a bunch of "tinkerniquers." We should be spending our time instead, says Knowles, as in fact Northcott does, on the development of suitable conceptual frameworks under which we can operate in the personnel management area.

Quite in harmony with the foregoing thought, psychologists have long bewailed the fact that in spite of the many years of effort devoted to the problems of selection research, typical examples of which are afforded by Briggs (35), Fleishman (74), Halsey (102), Konikow (141), Mandell (156), Meyer (166), and Stewart (205), little has this research contributed to the common body of psychological knowledge. Taking up the cudgels Travers (216) asks, "What should we do?" Why, says he, we "should seek to develop a model which will give direction to the search for variables still to be identified . . ." The model suggested in the opening paragraphs of this review, while obviously not the whole answer, can certainly lead toward the result which Travers and, in fact, all of us so earnestly seek.

But let us couple with this model, and with Travers' urgent plea, those thoughts suggested by Ghiselli when, as president of the Division of Industrial and Business Psychology of the APA, he asked all to consider the possibility of being more creative with respect to the "still-to-be-worked-out" concepts and problems in the challenging field of worker selection and placement. Consider, said Ghiselli (83) as did Wallace (224) in another context, the matter of criteria. These we manipulate technically but not conceptually. Except for occasional examples, provided this year by Jay & Copes (122) and by Rusmore & Toorenaar (187), the latter of whom ingeniously applied Brogden and Taylor's cost accounting concept to the selection of telephone operators, we continue, as per Bailey (10), Bair, Lockman & Martoccia (11), Bass (18), Harris, Howell & Newman (104), Hollander (114), Sluckin (201), Taylor & Hastman (211), and Tiffin & Hudson (214), to concern ourselves with, as Ghiselli said, matters of technique.

Ghiselli also regards it unfortunate that we look upon criteria as fixed and static when in reality they may prove to be most dynamic. Or how else could one explain, asks he, as does Gitlow (88) with respect to wage criteria, the fact that the validity of a set of test scores may vary according to longer or shorter periods of time during which performance records are obtained? This being the case, who is there now to say or give advice as to how we determine which period of time and which correlation between test and criterion, tell us something real about validity?

Next, what about the fact, asks Ghiselli, that a criterion can be individ-

ual in nature in the sense that two different workers might arrive at the same over-all result in two markedly different ways? For example, if sales volume is our criterion can we really treat as equivalent, as do Baier & Dugan (9) in their elaborate correlational study, two life insurance salesmen who sell different proportions of ordinary and weekly premium insurance? Psychologically, we don't know and this is Ghiselli's complaint.

Another problem discussed by Ghiselli is occupational analysis. How should we group workers? What characteristics determine similarity among jobs or positions? How shall we organize any number of these into classes? In terms of formal administrative categories? In terms of learned skills? In terms of personal characteristics, abilities, and traits? In the manner suggested by Roe (184)? And after we get through how about the workers themselves? Will they view jobs phenomenologically the same as we have classified them? And if not, how will these different phenomenological concepts affect test validity (81, 82, 84, 99)?

And so far, says Ghiselli, all of us have been talking about what we call independent jobs, jobs in which performance has little or, usually, no effect upon that in another job. So what about the case, asks Ghiselli, in which the incumbent can do his work only in interaction with another person, e.g., the pilot, bombardier, and navigator? Or even more difficult than this, the co-ordinate jobs of riveter and bucker-upper. How, asks Ghiselli, do we evaluate the performance of one of these without taking into account the performance of the other?

Having asked these pregnant questions Ghiselli himself launches into a number of projects designed to get some of the answers. And one of the most significant of these was his "differentiation of individuals in terms of predictability" (80). Finding that occupational level affected the accuracy which which he could predict the success of taxicab drivers Ghiselli asked, "Why not set up an employment procedure in the following order?" First, after we have interviewed our candidates in accord with well-known principles enunciated again this year by Adkins (3), Anstey & Mercer (5), and Uhrbrock (217), determine their occupational level. Second, exclude from employment those high thereon as these are the applicants for whom accurate predictions cannot be made. Third, reject those who fail to pass their selection test. In this way one can be sure, said Ghiselli supported by appropriate cross-validation data, of greatly increasing among those hired the proportion of potential successes.

Long ago Ruml tried to teach us that the object of any selection device was to enable us to divide applicants basically into discrete groups, not into continuous gradations. In other words we want to know whom to employ and whom not to employ. To make this decision we need to know which of our applicants will be successful and which will not. Degrees beyond this need not, as Dvorak (66) correctly points out, concern us. This being the case, not only do we, many times, not need a correlation coefficient, but find, frequently, that it is irrelevant and misleading. Therefore, we shall have

to question rather seriously the meaning of Ghiselli's ambitious and laudable attempt (81) to summarize, in terms of average correlation coefficients, what we now know about selection test validity for different kinds of jobs, positions, and situations. When will we learn that the validity of selection tests must be given in terms of expectancy tables such as those long advocated by Bingham? And next we turn to examples of a few other things which we might learn.

Randle (181), having access to records for 3000 business executives, 1427 of which he studied intensively, divided these executives into three groups: into those who could be promoted, into those who were satisfactory now but who were not promotable, and into those who were inadequate in their present positions. He then proceeds to show how those in the first and third of these groups differ from each other and alleges that the differences he discusses should be useful in the selection of future executives. Unfortunately for Randle, as well as for Bengt (21, 22) who made a similar study of salesmen, for Balinsky & Shaw (12) who made a similar study with the Wechsler Adult Intelligence Scale on 39 "top management personnel," for Jenkins (123) who did likewise with accident repeaters and nonrepeaters, as well as for the rest of us, this simply is not true. Not true because, as Randle himself points out, all data in his possession pertain to persons who at the time of testing were executives. Therefore, because of attrition, failure, change, mobility, and transfer, many who should have been included in the sample were not tested. Furthermore, it looks very much as if criterion ratings and trait ratings were, in many instances, made by the same persons. And finally even if there were no other faults there can be no assurance from Randle's data that contrasts among different groups of active executives will serve to identify potential successes and failures among those yet to become executives.

Let us note a study by Peck & Parsons (177), because in attempting to develop a projective type of analysis to be applied to an application blank, they made use of the records of very small numbers of workers, i.e., from 9 to 26. More serious than this, like Kirkpatrick (136), they studied only the records of men on the job six months to several years, a procedure which leads to findings not very apropos of the employment situation in which choices must be made from among those who present themselves for employment.

But now that we have pointed to a few difficulties is there not also a brighter side? Yes, there is and in reporting it we shall first cite studies giving negative results and then go on to those of a more positive nature.

Samuelson & Pearson (188), in a study of Kuder preference profiles, found no differences between survivors and dropouts in a trade school; Wallace, Clarke & Dry (225) found that the much publicized AVA test had no validity for predicting either first, or second year success of life insurance salesmen; Decker (59) and McCormick & Middaugh (159) found that although selected items could be made useful, the regular keys for the test

"How Supervise?" had no validity for the prediction of supervisory success; and Baier & Dugan (9) found that scores on Wesman's Personnel Classification test failed to differentiate, in terms of sales, between better and poorer debit life insurance salesmen.

A study which gave both negative and positive results is that reported by Hughes, Dunn & Baxter (119) who show conclusively what happens to the validity of a selection test when managers urge applicants to give specified item responses. They show that when managers or others know the scoring weights to be applied to certain items on a personal history form many of them cannot resist the temptation to tell an applicant how he should respond. And in doing this they obviously destroy the validity of the help that they say they so desperately need. And this type of cheating is, by the way, much more prevalent and is of more serious concern than that in which applicants indulge, unconsciously in accord with response set, see, e.g., Fricke (77); or consciously in the manner reported by Rusmore (186) and by Gordon & Stapleton (94), and which motivated Bass (16) to develop his "multiscale proverbs test."

In the category of those studies which gave positive results we may cite those by Elliott (68) who, like many others who should know better, presents his results in terms of proportions of successes and failures who have stated characteristics (such as high school education) rather than in terms of proportions of those having specified characteristics (e.g., different degrees of education) who achieve success and failure; by Patterson (176) who reports on "the prediction of attrition in trade school courses"; by Bennett & Gelink (23) who revised their well-known short employment tests; by Crawford & Crawford (57) and by Osborne & Sanders (172) who report on the value of a small parts dexterity test; by Briggs (35) who gives a popularized account of how General Electric utilizes tests to select executives; by Hughes (118) who reports that upon the basis of a classification of applicants into three Murray need categories: dominance, altruism, or gregariousness, he can predict first year production of life insurance agents; by Scollay (192) who used as a criterion for the validation of various personal history items the difference between present salary and that earned on first job; by Guba & Getzels (100) who, using Kuder's Preference Record and finding no differences between scores or patterns of rated and nonrated air force officers, did feel that they found differences between all officers and "men-in-general," and, using the Allport-Vernon-Lindzey Scale of Values, between air force officers and college students; and, finally, by Witkin (237) who redemonstrated two well-known and long-established facts. First, that on Strong's Vocational Interest Blank, the application of which Darley & Hagenah (58) describe in a carefully worded monograph, life insurance salesmen and sales engineers can be (as Ream long ago found) quite significantly differentiated. Second, that this differentiation shows up on a group of scales which, we know from Strong's research, supplemented this year by an 18 year follow up study (209), are highly intercorrelated.

We have in the immediately preceding paragraphs been considering, in a sense, just one aspect of a much larger field, that of occupational mobility. In this broader sphere we may note that Pihlblad & Gregory (180) demonstrated that intelligence as such is not the primary factor leading to geographical migration. This in spite of the fact that migrants do, in general, have higher intelligence test scores. It is rather, said they upon the basis of an analysis of appropriate records for 5000 Missouri young people, that those of higher than average intelligence are differentially attracted to certain occupational callings. And it just so happens that the outlets permitting the satisfaction of these callings occur more frequently in the larger rather than in the smaller cities, the latter often being the original residence.

According to Ghiselli initiative is a second factor of importance which operates to differentiate between those who elevate themselves into the higher occupational levels and those who remain at the lower levels. On a scale developed to measure initiative (79) he found, between age and scores thereon, a positive relation for persons at high occupational levels, and a negative relation for those at the lower levels.

In several articles published in the *Journal of College Placement*, [see Abbott (2), Dickinson (62), and Endicott (69)] two ideas are prominently emphasized. First, that in achieving job success and therefore job stability, certain correlates of which in the earlier manner of Craig and Hoopingarner were this year studied by McNaughton (163, 164), attitude is most important. And second, that the pre-employment description of one's job duties is a most important correlate of this attitude. This is a verification, if one be needed, from questionnaire studies of a fact that Weitz (232) so neatly demonstrated experimentally.

We mention in passing only, Warner & Abegglen's ambitious study (227 to 230) as a result of which they suggest that, as compared with conditions 25 years ago, there is greater occupational mobility from the lower to the upper occupational levels, and we find ourselves preferring Newcomer's much more critical study (170) of the origin and background of "The Big Business Executive."

Although not mentioned by Warner & Abegglen, nor by Newcomer, a potent factor in turnover among management personnel is, undoubtedly, the matter of opportunity afforded for training and personal development. During the year and headed by Schleh's "Successful Executive Action" (191) in which general rules of action are proposed, McLarney's "Management Training: Cases and Principles" (161), and Uris' "Developing Your Executive Skills" (219), there has been the usual plethora of "how-to-do-it" or "why-you-should-do-it" publications and, as has become traditional, we can group these into the three areas of management development, supervisory training and, for the "run-of-the-mill" employee, skill acquisition. Apparently only employees can acquire skill, only supervisors can be trained, and only managers can be developed.

Management development can be approached, reports Hay (106) some-

what skeptically, via the Bethel way; says Blansfield (30), via the conference technique; says Bennett (24), via the lecture technique; says Youmans (239), via an internship program even though, says Wilson (236), and with specific reference to industrial psychologists, there are difficulties; or, say Jennings (128) and Argyris (6), both of whom point out problems, via group training.

Although Jennings (124 to 127) and Harris (105) give several ideas on the benefits of group training, Jennings (128) is nevertheless apprehensive lest we run the danger of suppressing critical thinking. This, because of our well-known tendency to conform to majority opinion. And so in emphasizing social group skills as a result of which, say Klein (137) and Gordon (95), group-centered leadership is supposed to emerge, are we not apt to develop, asks Jennings, "trainees," and not "critical independent thinkers"?

By whatever general method we choose to proceed we shall have to consider, say Bennett (25), Hoslett (115), and Houston (116), the last of whom pleads self-acceptance as a prerequisite to self-development, a number of questions: Who should be selected for participation in such a program? How should he be conditioned to accept instruction in knowledge and theory concerning that which he is to learn? And, how should one effect his necessary "on the job" training? One might do these things, according to Sherman (199), by making an appraisal of each member's job performance and future potential, by taking an inventory of managerial resources, by replacement planning, and by development of each member of the management team. And, for each of these areas, set up, as was done at Kelly AFB, an appropriate committee. Then give to each committee the authority it needs to make all policy in its area, to decide who is to be included in the program, to specify methods of procedure, and to review all appraisals. Finally, appoint for each committee a coordinator who will assist the committee, maintain records, and keep it posted on progress.

Moving next to the supervisory level we find that Guest (101) properly questions much that is passed off as foreman training, and we learn, from Korb (142), how training needs can be determined; from Brown (38), various "democratic" ways of proceeding; from Castle (50), Korb (143), and Mandell (155), how to evaluate results; from Hersey (110), how to make the foreman, whose duties and attitudes Walker (222) and Foa (75) carefully describe, part of management; from Barthol & Zeigler (15), if we wish to dispense with such a frivolous gesture as using a control group, how changes in answers to the questions in "How Supervise?" indicate that supervisory training can be effective; and from Speroff & Heydrick (204), if we wish to use a loaded questionnaire, that human relations training can have the most beneficial results. Bass (17) reports an attempt to predict supervisory success.

Bauer (19) suggests, without critical discussion, that if one desires a learning curve for a salesman's performance all one needs to do is to "accumulate and average the sales for each month for the total number of

months to date." This value will, says he, and for a period of time not to exceed 15 to 18 months, approximate a learning curve, of what type, however, he did not say. In a more critical study Taylor & Smith (212), after analyzing some 70 learning curves for 12 power sewing machine operators, suggest as an appropriate prototype a negatively accelerated curve, with a sharp initial rise and a more gradual and nearly linear increase thereafter.

But even if we know the shape of an "industrial learning curve" or know, as Siegel (200) demonstrates, that training is effective, we still need to know the best methods of acquiring skill which, as Burack (40) shows, is a major problem to an extremely high percentage of combat casualties. For rifle marksmanship MacCaslin & McGuigan (152) suggest that the whole method is the best—this on the basis of their discovery that results of target practice can better be predicted for students who have learned via the whole method rather than via the part method. But Seymour (194) after studying, on capstans, the relative efficiencies of a whole method, an isolation method, and a part method, comes up with the idea that the most important thing is to see that the task to be learned contains elements which, during practice and thereafter, are perceptually stringent.

Now what about factors other than methodology which may influence productivity or rate of learning? On this latter point we may note a most significant study on ability grouping by Findlay, Matyas & Rogge (72). Having tested a group of potential army trainees they arranged three class room set-ups: one which included a mixture of men of high, medium, and low ability; a second which included men of low and men of high ability; and a third group which included only men of low ability. Then using as a criterion a four hour performance test, they found that they had to reject their original hypothesis which was to the effect that low ability men would profit by being in the same class with men of high ability. The only significant differences were between men of high, medium, and low ability, regardless of class room grouping.

In bringing to a close this review we may note that industrial psychology, having been in existence for 55 years, is beginning to be conscious of its history. During the year Kavruck (132) detailed the high lights of 33 years of test research in the U. S. Civil Service Commission, and in the process reminded us that Thurstone was among the first who made contributions in this direction; Donovan (63) discussed 50 years of "The Civil Service Assembly"; and Patterson (175) did the same for many years of research aimed at predicting success in trade and vocational schools. Wallace & Twichell (226), in a review which emphasized not only the interdependence of much of the effort being devoted to the understanding of better methods of recruiting, selecting, post-selecting, training, and motivating personnel, but also, the several ways in which research contributes to the process of management decision-making, recounted some 10 years of "purpose and accomplishment in life insurance distribution research," research which, while

in some respects recent, has its roots deep in the very beginnings of industrial psychology.

Gilhooly (86) gave us an account of the Airman Career Proficiency and Air Force Job Knowledge Tests of the United States Air Force; Dvorak (67) described the usage, in 27 foreign countries, of the USES developed General Aptitude test battery; Carter (47) reviewed and summarized 25 years of "field research" in journalism; and Tiffin & Prevratil (215) summarized the extent to which psychologists have been accepted in and are utilized by the aircraft manufacturing industry. Nadworny (169) described the results of many years of the application of Fred Taylor's brain-child, scientific management; Brown (37) and Christie (52) sketched labor history, the latter with specific reference to the carpenter's union, the former with respect to the International Ladies Garment Workers Union but only for the purpose of showing that, from 1900 to 1950, the size of this union could possibly, although not unequivocally, be considered "as a function of intra-union conflict." And finally Ferguson (71)—I've got to get my name in here somehow—in his 1956 address as president of The Division of Industrial and Business Psychology, traced from its beginnings, by Walter Dill Scott in 1902, many of the major developments in the field of industrial psychology.

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ENGINEERING PSYCHOLOGY

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Since this is the first time that a chapter of the *Annual Review* has been devoted to engineering psychology, I considered allotting some space to historical background. However, the volume of current publications is such that I decided instead to include only articles published between January, 1956, and May, 1957, inclusive.¹ With respect to quality I have been less selective in certain areas than in others in order to give a comprehensive picture of the current status of the field. I include unclassified Government reports, since nearly all of these are now available to scholars. However, I exclude all privately distributed reports.

A brief statement about engineering psychology is necessary in order to explain the rationale for the selection of the topics covered by the present review. Engineering psychology has both a professional and a scientific aspect. The professional aspect involves the application of psychological knowledge to the design of human tasks, man-operated equipment, and man-machine systems, usually in collaboration with engineers. I devote a small section to these activities, but the majority of space is allocated to the more scientific aspects. One reason for this unequal emphasis is that very little is being published regarding strictly applied work. In contrast, many publications in experimental psychology are relevant to engineering psychology. In fact, the scientific data supporting the professional side of engineering psychology are contributed mainly by conventional areas of experimental psychology, such as vision, hearing, perception, and learning.

All the biological and social sciences have contributions to make to the design of machines and man-machine systems. I have not attempted to cover contributions other than those of psychology. Nor have I included psychological research which deals with organismic or procedural variables in relation to performance, if it does not involve machine or system design variables, or have implications for design problems.

PROFESSIONAL ACTIVITIES

Compilation of handbook-type data.—In an effort to meet the demand for assistance on applied problems, engineering psychologists have undertaken to compile data for use in handbooks or other publications having the general format of the *Handbook of Chemistry and Physics*, or the *Handbook of Instructions for Aircraft Designers*. The *Tufts College Handbook of Psycho-*

¹ The publication date on a Government publication usually corresponds to the date of acceptance of the manuscript and not to its date of publication; since I include here only articles bearing a 1956 or 1957 date, a considerable number of Government reports actually released in 1956 but dated 1955 are not included, and similarly, very few of these reports bearing a 1957 date are included.

physical Data, published in 1949, was the initial effort in this direction. The *Human Engineering Guide for Equipment Designers* (edited by W. E. Woodson, 1954) was a subsequent contribution. During the past year a Joint Services Steering Committee, under the chairmanship of Max Lund, coordinated efforts to provide a comprehensive *Human Engineering Guide to Equipment Design*. Work on 12 chapters is under way. Preliminary versions of two chapters, both drafted by Ely *et al.* (50, 51), were released as separate reports.

Van Cott & Altman (142) discuss procedures for including human factors in weapon system development, while Folley & Altman (59) offer a guide to the design of electronic equipment for maintainability, and Hunter (87) provides a general guide for designers of such equipment. The problem covered by these publications is that of designing computers and other electronic machines so that men with only moderate amounts of training can locate sources of trouble in such devices, and make necessary repairs, adjustments, and calibrations.

The magazine *Electrical Manufacturing* carried a series of features on human factor applications to equipment design problems. Ten of these are available in combined reprint form (see 61).

Taylor (139) discusses the relations between technological and scientific aspects of engineering psychology, including systems evaluations, and gives a critical evaluation of the area. Wissel & Hall (147) discuss the relative roles of the Government and the university laboratory, private research and consulting firms, and of industry, in human engineering research. One trend in publication practice is to include an "engineering applications" section along with the technical section reporting a series of investigations. Kraft's (96) report on a lighting system for radar centers, for example, contains 32 pages of detailed explanations and instructions written for engineers in handbook style.

Journals and societies; professional training and responsibilities.—The Ergonomics Society announces publication of an *Ergonomics Journal*, with A. T. Welford of Cambridge University, as editor, assisted by an international board of consulting editors. The subject matter will include, but not be limited to, engineering psychology. A Society of Engineering Psychologists, a division of APA, has been organized. The American Society of Mechanical Engineers, and the Institute of Radio Engineers have established professional groups on human engineering, and a Human Factor Society has been formed which is intended to bring together various professions interested in human-factor design problems.

The usual number of growing pains of a new professional area are evident in articles by Bromiley (30), Goldsmith (66), and Warren (144) who are psychologists, and by Mandell (109) who is Editor of *Research and Engineering*. Warren discusses training courses in human factor engineering. Problems of certification and licensing are also being discussed.

The over-all problem of applying biological, psychological, and social

science data to the design of man-machine systems clearly involves many professions. The present writer prefers the use of specific terms such as engineering psychology to identify the scientific or professional roots of the various contributions to this area, and an over-all term, such as human-factor engineering (or just human engineering), to identify not a profession, but the over-all topic or problem area. The realm of engineering psychology research is one to which the experimentally trained psychologist makes major contributions; the application of psychological facts and principles to the design of machines and systems is a realm of professional responsibility regarding which there is likely to be continued and lively discussions for some time to come.

SCIENTIFIC TOPICS

The characteristics of engineering psychology research.—Although the aims that characterize the types of research studies identified with engineering psychology have not been explicitly listed in published reports, they appear to be twofold: (a) to provide quantitative data on human performance characteristics, for populations of people, as a function of task variables, and (b) to provide the kinds of measures of independent and dependent variables which will permit quantitative specification of the levels of reliability, operability, safety, training costs, or other performance criteria of interest for machine and systems design.

A task variable is any characteristic of the stimulus, the response, or of the simulated machine which affects the nature of the task to be performed by a human operator and which can be contrasted with procedural (conditions of training) or organismic (conditions of the organism) variables. The study of task variables is the primary topic of engineering psychology, including the interaction of task variables with other variables.

The other aim of engineering psychology research is primarily a matter of the validity of research methods and measures. Two examples of studies that have used highly meaningful measures of experimental variables can be cited as a means of illustrating this aim. Baldwin & Nielsen (11) studied the subjective sharpness of colored TV pictures as a function of the relative subjective sharpness of the red, green, and blue images used to produce the composite picture. However, they were not content simply to report that subjective sharpness of the green image was much more important than that of the other two components in determining the judged sharpness of the composite; by a series of steps they were able to calibrate the sharpness of the photographic transparencies used in their laboratory studies in terms of the corresponding band width of the electromagnetic spectrum required to transmit pictures of equivalent image sharpness. Thus, not only could they conclude that "sharpness is maximized by apportioning about two-thirds of the total bandwidth to the green primary picture" (p. 685), but they could state the exact saving in bandwidth (about 50 per cent) that can be achieved by such an allocation over an allocation that gives equal weight to each component, subjective sharpness remaining constant. Schipper *et al.*

(126), by similar conversion of the basic time data obtained in their system studies, have been able to report the effects of using different displays, procedures, etc., in units of both absolute and relative pounds of fuel saved on the average by each aircraft handled by the system. Both of these studies, permit a dollar value to be placed on certain engineering design variables, without any loss in the value of their data for purely theoretical purposes.

Cross-validation.—One recent sign of healthy self-criticism in engineering psychology research has been the appearance of validation studies relating the results of laboratory experiments to the results of similar tests conducted in the field or on actual operational equipment. Churchill (39) compares the use of a photographic projection method with the use of actual instrument dials and finds neither practical nor statistically significant differences in errors or time of readings for the two methods. Bessey & Machen (19) report confirmation under field-test conditions of earlier work of Smith & Boyes (135) on the effects of screen brightness and ambient illumination on performance of radar operators. Besnard & Briggs (18), in a carefully controlled study in an Air Force maintenance school, find no significant difference in total number of errors made by two groups, one trained on operational equipment, the other on a fire-control system simulator. Peryan & Haynes conclude that "... pretesting of ratings in the [Quartermaster Food and Container] Institute laboratory may be considered equivalent to pretesting by the soldier-consumer method" (116, p. 6). Examining the reliability of field-testing methods, however, Zweigbaum (148) shows that the major component of variance in results of tests of electron tubes can be attributed to field operator variability in using the test set.

One of the very general methodological issues facing engineering psychologists in the aviation field is the validity of laboratory findings for prediction of pilot performance in using operational equipment. Current research programs described by Mayo (110), Ritchie & Baker (120), and Williams *et al.* (146) offer considerable promise of progress both on this methodological question and on the development of much-needed principles for achieving an integrated cockpit display.

In the present writer's opinion a major deficiency in criterion research results from the lack of studies examining the reliability of the opinions of equipment users (such as test pilots) and of design "experts," and testing the validity of their opinions in relation to more objective criteria. Consumer preference research is already accepted as a phase of engineering psychology; it remains to determine the extent to which greater use can be made of so-called "expert opinion" or the extent to which such opinions are currently being misused, in the case of equipment that is being designed not so much to please the customer as to meet other human-factor criteria such as safety, efficiency, and personnel and training requirements.

MAN-MACHINE SYSTEMS

Field studies and rational models.—A rapidly growing interest in systems

design and operational problems has led to three kinds of recent activities: (a) the analysis of existing system operations, (b) the application of mathematical models (linear programming, cueing theory, graph theory, etc.) to man-machine systems, and (c) laboratory studies using simulated machines, operated by teams of real people. The second of these topics is being pursued actively by operations research (OR) teams. A comprehensive presentation of this approach to systems problems is given by McClosky & Coppinger (105) in a book to which 29 authors contribute 13 case studies (histories of particular system analyses) and additional sections on methodology and information-handling by groups of people. Ackoff (1) describes the phases of an OR project and discusses the development of operations research as a science, citing 153 references.

Most engineering psychologists have tended to emphasize empirical studies of existing or simulated systems, either in the field or in the laboratory, rather than to seek answers via the use of mathematical models. A number of field studies have been reported. Conrad (43) analyzes problems in a mail-distribution system and relates these to his research on timing and pacing of skills. Bean *et al.* (14) report results from the collection of extensive data on airport communications and control system activities. Miller (112) analyzes the histories of two military weapon systems and shows how it should be possible to forecast personnel requirements at an early time in the engineering development cycle.

Laboratory studies of systems.—Laboratory investigations of man-machine systems have been undertaken by a few groups. In these studies three kinds of independent variables have been manipulated: (a) information or display variables, (b) organizational or procedural variables, and (c) input or system-load variables [see Schipper *et al.* (126)]. Probably the most extensive system simulation for human-factor study to date is that carried out by the Systems Research Laboratory of the Rand Corporation and reported by Chapman & Kennedy (36). These authors develop a theoretical view which emphasizes the ability of people, when under task-load stress, to learn to modify their procedures and to allocate system time to various activities in such a manner as to maintain a high level of performance on critical aspects of the over-all system output.

Schipper and associates (124, 126, 127), in the Ohio State University Laboratory of Aviation Psychology, report the results of the first four of a series of system experiments oriented toward the problems of air traffic control. The display of continuous information identifying all objects (aircraft) about which decisions must be made in operating the system is found to result in a large and significant improvement in system performance over a condition in which operators have to remember identities of aircraft. In contrast, the use of two different procedures for dividing responsibility among controllers was found to have no significant effect. It should be noted that these studies are conducted with sufficient replication (power) that considerable confidence can be placed in negative findings, which in them-

selves may be of great economic or engineering importance, since a negative finding often gives the design engineer much needed freedom in choosing among alternatives.

A close working relationship appears to be developing among the various groups interested in man-machine system performance. In particular, social scientists are beginning to give greater emphasis to group performance efficiency, in relation to task and organizational variables, communications networks, and type of information available. The studies of Lanzetta & Roby (98, 99, 122) and of Shaw & Rothschild (130, 131), dealing with communication nets and the distribution of information among group members, contribute directly to the design of efficient man-machine systems. Research on personnel and training problems relative to various new-type systems also provides data of direct importance for improved design of new systems.

Research on man-machine systems will undoubtedly increase in the next few years in spite of such formidable difficulties as those which arise from the fact that simulation often requires expensive computers, experiments are costly, and adequate theory to guide research is lacking. However, the need for this type of study is obvious, since it often is too late to effect changes in a system after the actual system becomes available for study under field conditions.

AUTOMATION

Increasing use is being made of computing and data processing machines on every side. A report by Gerlough (66), as an illustration, describes a small computer which varies the red-green cycle of a traffic light on the basis of (a) the number of cars waiting in the red lane, (b) the longest time any car has waited in the red lane, and (c) the instantaneous volume of traffic in the green lane. As Warren (144) points out, automata not only have replaced human operators in the performance of highly routine tasks, but are now being given greatly increased memory, a wide repertoire of subroutines, and sufficient programming flexibility that they are now beginning to replace human operators in the performance of much more complex, intellectual-type tasks.

Recognition of speech and handwriting by machines.—The utilization of high-speed data processing machines by men is seriously limited today because men must punch keys or operate some similar set of manual controls in order to communicate their questions or instructions to the machines. Considerable research effort is now being given to the development of devices that will accept the two forms of communication most commonly used by man—oral and written language. The people engaged in this research are keenly aware that scientists lack sufficient knowledge of the stimulus properties and patterns which form the basis for language recognition by man, and much of the research effort in support of the development of voice and character recognition devices is now being directed toward the study of some fundamental problems of pattern recognition.

Olson & Belar (115) discuss some of the problems of developing a voice-operated typewriter, and describe a demonstration device which will discriminate among and type 10 syllables. However, the present apparatus is not a practical device since it must be adjusted differently for each speaker. Fry & Denes (60) describe a series of experiments on the same topic.

The recognition of alpha-numeric characters by machines, particularly printed characters which have a fixed format, is a simpler problem than the recognition of speech. Greanias *et al.* (68) outline a logical approach to character recognition, using a sampling of subelements. Eldredge *et al.* (49) report some success in designing a device that utilizes a scanning procedure.

Theory of automata.—Shannon & McCarthy (129) have edited a volume containing contributions from 15 scientists dealing with such topics as artificial intelligence, programming theory, and thinking by machines. McKay (107) offers a general information-flow model of human behavior which reflects the influence of recent computing-machine developments on theory of how the brain works. The present reviewer predicts that in the next few years psychologists will receive considerable stimulation from mathematicians and physical scientists who would like to see psychologists increase the level of research on thinking, reasoning, and the higher mental processes. The computer specialist feels that if these human activities were well enough understood, machines could be built to perform similar higher level functions.

SKILLED PERFORMANCE

By far the largest number of articles of interest for engineering psychology published in the last year deal with tracking, information processing, and other forms of skilled performance in which rapid responses to stimuli are required. The number of task variables that influence the proficiency of a perceptual-motor activity is large indeed. Stimulus properties such as frequency, coherence, amplitude, and direction of motion; response properties such as the direction, amplitude, required force, sequencing and timing of movements; machine properties such as lags, noise (random signals), intermittance, and number of information sources, are only a few examples from a growing list of these variables. The difficulty of discovering optimum task characteristics, of specifying levels of human proficiency as a function of task variables, and of developing usable theory, is immensely complicated by the fact that many of these variables are being found to interact with each other. Because of space limitations, the present review includes a larger portion of newer work in this area; in particular, often only one or two of a series of integrated studies of established topics are included, even though the reviewer feels that such programmatic research is highly valuable.

Quickening a feedback signal.—Taylor & Birmingham have developed an important general principle for the design of closed-loop control systems which include a human operator. During the period of this review they have

published one general article (140) describing this principle. Although it is not possible to summarize their ideas adequately in a brief review, a general notion of the problem and the nature of the solution which Taylor & Birmingham propose can be indicated. In a typical closed-loop control task a man observes the output of a machine and attempts to produce a control signal which will subsequently produce some desired change in the machine output (such as a change in the depth of a submarine). To the extent that the machine responds sluggishly to control signals (i.e., possesses inertias) the man finds it necessary to anticipate these lags and to generate signals which compensate for the particular transfer function of the machine. This is very difficult. For example, if the machine output can be represented as the third integral of the input, then the man has to respond in a manner which is somewhat analogous to requiring that he perceive and respond to the third derivative of the observed output. Quickening consists essentially of providing the man, by some appropriate means, with a feedback signal which contains the output signal plus other selected functions of the output such as derivatives (or functions of his previous responses) so that his subsequent responses (control signals) can bear a simple proportional relation to what he sees. Quickening simplifies the perceptual and may simplify the motor aspects of a task, thus, in Taylor and Birmingham's terminology, permitting a man to act more analogously to a proportional amplifier. The resultant effect on the human controller, called "unburdening," is found to be highly advantageous in numerous specific applications, most of which are as yet unpublished. Ritchie & Bamford (121) report a beneficial application of the quickening principle to an aircraft turn indicator, and Holland & Henson (85) report that practice on either a quickened or unquickened system is beneficial on switching to the other type of system, although 100 per cent transfer is not realized.

Stimulus frequency and coherence.—In spite of their great interest in stimulus properties (especially in relation to sensory and perceptual processes), psychologists have often ignored the properties of the stimulus in studies of tracking or other perceptual-motor tasks. They have used arbitrary and unspecified (cam generated) target courses, arbitrarily chosen periodic signals, and opportunistic sequences of movement patterns, and assumed that their results would generalize to other more or less coherent (predictable) stimulus or response sequences or both. Fortunately, the importance of frequency, in the case of periodic signals, and of the power spectrum of the stimulus, in the case of stochastic signals, is now being recognized. Hartman (77, 78) reports, for both pursuit and compensatory tracking, that tracking error increases progressively as target frequency is increased from 10 to 60 c.p.m. Chernikoff & Taylor (37) report a similar effect of increases in frequency for both pursuit and compensatory tracking, whether they use a displacement, a rate, or a rate-aided tracking system, as do Bowen & Chernikoff (22) for a rate-aided system, and Chernikoff *et al.* (38) for a simulated aircraft system. More important, however, all of the

last three studies reveal important interactions between frequency and other task variables. The optimum aiding constant shifts upward with frequency (37). Although a pursuit display is generally superior at higher frequencies, the difference becomes less and it may become inferior to a compensatory display at low target frequencies when a rate or a rate-aided system is used (22, 37), or when the machine system involves lags such as is the case in controlling an aircraft (38).

Kaestner & Grant (89) interpret differential transfer of training effects between a periodic and a random tracking task in terms of specific learning of the periodic function. In a study which in part bridges the gap between traditional concept formation research and tracking research, Detambel & Stolurow (48) view concept learning as a sequential process and find that the lower the temporal synchronization of relevant and irrelevant cues the more efficient the learning. Briggs & Fitts (26) report large and progressive increases in tracking error as a function of the ratio of noise (random component) to feedback signal in a simulated aircraft control task. However, Briggs *et al.* (28), although observing comparable effects on performance during learning trials, find no significant effect on learning as measured by transfer trials with a uniform visual signal-to-noise ratio.

Machine lag.—Lag is a rather loose term, since the output of a machine may be related to its input in many ways (specified precisely by its transfer function), all of which may involve some type of delay between input and output. Conklin (41) finds that in controlling a system which gives an exponential output to a step-function input, human performance becomes progressively worse as the time constant of the lag is increased. When two such exponential delay networks are placed in series the output to a step-input becomes an S-shaped function. The latter type of lag is much more detrimental than a simple exponential delay, even when the time to reach all but $1/\epsilon$ of the final output value is the same for the two networks. Briggs *et al.* (29) report comparable changes in difficulty in comparing lags produced by single and double stages of integration, and once again find a significant retardation of learning from practice on one lag, as measured by initial transfer to a different type of lag, but the effect is of short duration.

Fleishman & Hempel (56) although concerning themselves primarily with factors accounting for individual differences, identify numerous task characteristics that may also account for the difficulty of various skilled activities. Among their list of factors, the ones that are manipulable by the design engineer include (a) the extent of use of fine adjustments, (b) the degree of use of gross movements involving several body members, (c) the use of adjustments requiring manual dexterity, (d) requirements for controlling rates, and (e) spatial and (f) orientational aspects of displays and controls.

Intermittency.—Performance in serial or continuous tasks in which the stimulus (input plus feedback) is present intermittently, or changes in discrete steps, is of interest for two reasons: (a) people often have to use

machines that provide quantized or intermittent information, and (b) performance on this type of task throws light on certain theories of human periodicity or sampling behavior. Poulton (118) reports a simple but ingenious experiment in which either the input (target), the output (cursor), or both, were viewed intermittently at different frequencies by the subject in a pursuit tracking task. An intermittent display was more detrimental in tracking a complex target course than in tracking a simple periodic target motion; interference with the visual information provided by the input (target) was more detrimental to performance than was interference with the feedback (cursor) signal. Bennett (15) finds that performance in tracking with sampled data is poorer at lower sampling rates and concludes that performance is proportional to the sampling rate raised to some power. Senders (128), using a dual compensatory tracking task, illuminated the two displays intermittently. The maximum in performance was found in the vicinity of 40 exposures per minute, but even at this optimum sampling rate performance was poorer than for the continuous exposure condition.

Klemmer (92) studied tracking accuracy as a function of target position uncertainty in a task in which the target moved in discrete steps. He estimates the maximum information transmitted to be about 4.2 bits/sec. for a one-dimensional and 6.6 bits/sec. for a two-dimensional task using motor responses. This performance is slightly higher than that reported by Harlow & Butler (74) for monkeys who operated 11 knobs in response to a series of discrete light stimuli, but is comparable to information rates (4.5 bits/sec. for self pacing, about 4 bits/sec. for automatic pacing) reported by Alluisi & Muller (4) when using discrete numeric stimulus codes and key-pressing responses.

Time uncertainty, timing, time sharing, and temporal pacing of performance.—There is a solid background of historical data on topics related to the timing of responses. We can attribute this fortunate state of affairs largely to the fact that many of the early studies of motor skill, such as those of Stetson and of Seashore, were motivated in part by an interest in music and rhythm. Recently Conrad (42, 44), acknowledging the influence of Sir Frederic Bartlett in this area, has been conducting a series of studies of timing, and of the regulation of self-paced periodic activities. He finds (42), for example, that when stimuli occur in a fixed temporal sequence within a periodic task, the subject will often utilize a different temporal sequence for his responses, one that permits what is for him a better allocation of time to the different elements of the task.

Studies of the age of workers in relation to their ability to work at paced vs. unpaced tasks have been emphasized at the Nuffield project on aging at Cambridge University. Brown (31), Shooter *et al.* (132), and Crossman & Szafran (46) find that older workers are handicapped by speed requirements, but adjust to speed stress more effectively if the task permits greater latitude in the organization and timing of component activities.

In a rather novel finding, Klemmer (95) reports that subjects can re-

spond in synchronization with a stimulus rate of two per second only when there is some uncertainty in the stimulus sequence. For example, when discrimination among a set of possible lights was not required, (i.e., when the subject knew the sequence) an irregular temporal distribution of responses resulted. In a further study of time uncertainty in simple reaction time Klemmer (94) reports that it is not the average duration of foreperiod which influences reaction time, but rather the variability of successive foreperiods. The less variable the foreperiod the shorter is reaction time, independent of the length of foreperiod, within limits.

Mackworth & Mackworth, studying ability to respond to overlapping signals (time sharing) in a complex task, conclude that "the greatest drawback of multichannel displays is believed to be their tendency to give rise to momentary but very damaging peaks of speed-stress" (108, p. 47). Alternative human-engineering solutions to the problem of overlapping signals were tested by Bertsch *et al.* (17). In one scheme selected incoming messages could be stored until the operator called for them. This scheme was superior to a fixed delay scheme and to the no-delay condition for all criteria when used by a small problem-solving group.

In a simple information transmission task, Klemmer (93) finds that forcing a subject to alternate between a visual and auditory input signal more rapidly than once every 2 sec. lowers over-all performance, resulting in a relatively greater decrement on the easier task.

Stimulus-response compatibility.—Investigators continue to find that a highly significant task characteristic is the congruence of stimulus and response patterns. Such factors as correspondence of the direction and planes of motion, the organization of elements, and the linearity or circularity of stimulus and response patterns are found to interact, so that performance is a function of the degree of spatial or of learned correspondence of input and output codes. No general theory has emerged in this area; the most typical explanation of findings is couched in terms of response sets [see Lovelace, (104)]. Typical studies are those of Anderson *et al.* (6), who report that the spatial location of stimulus lights and response keys relative to the position of the subject's body has a much smaller effect on performance than that found earlier when noncongruent relations between stimulus and response patterns were used. Green *et al.* (70) continue a series of studies dealing chiefly with variations in control location and direction (or plane) of movement. Graham (67) notes that the differences in verbal responses to horizontal, vertical, and circular scales are similar to differences reported earlier for motor responses to such displays. In one contribution, the writer refers to the importance of the "... organization of both stimulus and response fields in a congruent manner" (57, p. 312) as if this were a novel idea.

Oral vs. motor responses.—Alluisi and associates publish several studies [(4) and (5) are typical] in which large differences in performance speed and accuracy are found with given sets of stimuli, depending on the use of oral (naming) or motor (finger pressing) responses. In most of the comparisons

oral responses gave superior performance, but the authors, pointing to opposite results in some of their earlier experiments, attribute the results to compatibility effects, and hypothesize that in any particular task the relative effectiveness of a verbal as contrasted with a motor response mode will depend on the set of stimuli used. Katchmar *et al.* (90) compare verbal and motor responses with a single stimulus coding set (a horizontal row of lights) and report several differences in the types of errors made with the two response sets.

Stimulus and response amplitude.—Identifying their variables by such names as control sensitivity, control-display ratio, and control length, several investigators have examined the effects on tracking performance of varying the amplitudes of required motor responses, or the amplitudes of stimulus changes occurring on their displays. Hartman (79) finds a U-shaped function for control amplitude, 18-in. movements being optimal, 27-in. movements significantly worse. Andreas *et al.* (7) and Rockway *et al.* (123) find better performance with large movements, but as is the case in other learning studies reviewed here, find large positive practice effects on transfer to different amplitudes.

The present reviewer suggests that amplitude functions may vary markedly with different stimulus frequencies. Also, it is important to recall that previous studies indicate that control and display amplitude functions often exhibit independent characteristics which may be obscured when results are interpreted only in terms of control-display ratios.

In a psychophysical judgment task, Schipper & Versace (125) found that judgments of the collisions of two target tracks on a simulated radar scope were equally accurate with a 10- or a 20-in. display, with either fuzzy (somewhat out of focus) or sharp targets, and with either of two sizes of simulated radar blips. They interpret these results as indicating that subjects are making ratio judgments in a range not affected by the differences in absolute size.

Analysis of movement components.—Analytic studies of movement components, especially in repetitive and manipulatory tasks, reveal many of the characteristics of the organization of skilled activities. Smith and his associates contribute three additional studies in this area. Huiskamp *et al.* (86) find that 11 days of practice do not eliminate the initial superiority of visual plus proprioceptive control over nonvisual control of movements. Hall (72) and Simon (133) criticize an "atomistic" concept which they attribute to some proponents of time and motion economy—"elements" of a task require varying amounts of time depending on preceding and following elements and on available perceptual cues.

Herbert (82) finds positioning movements to be more accurate along the Y axis than along either the X or the Z axes.

Davis and associates report 12 studies investigating the ways in which muscular tension, as measured by electromyographic techniques, affects performance in control tasks [see Davis (47) for a summary and a listing of

the other reports in a series]. Emphasis is upon muscular response patterns and their interactions.

Taylor & Smith (141), in a type of study that is all too rare in psychological research, analyze learning curves extending over a period of 100 weeks in an industrial task (power sewing machine operation). In agreement with some other scattered evidence they find that improvement in performance continues for very extended periods, and speculate that the supplanting of visual by proprioceptive control is a very gradual process.

Proprioceptive factors (control loadings).—Briggs *et al.* (27), in a complex tracking task, found that a reduction of either force or amplitude cues over those found in a typical aircraft control system resulted in lowered performance scores. Once again, however, these variables were found to have little or no effect on learning as measured on a transfer task. Lincoln (101) trained subjects to turn a handwheel at a constant rate and gave knowledge of error in various ways at the end of trials. Verbal and kinesthetic cues regarding error were equivalent. Gerall *et al.* (64), using a discrete task, note detrimental results for coulomb friction, in agreement with earlier studies. Performance in a continuous task (moving targets) gave ambiguous results.

Control knobs.—Bradley & Arginteanu (24) report another in a series of studies of the ability of subjects to identify and operate controls having different types of knobs. In this instance, reaching time and turning time were analyzed as a function of knob diameter. A 2 in. diameter is optimum, but a 1 in. diameter can be used if space requirements make it desirable and frictional torque is moderate.

The eye as a control mechanism.—From time to time there is speculation as to the suitability of the eye as a device for generating a control signal in a tracking task. The practical obstacle in the way of using the eye in this manner arises from the difficulty of securing a continuous signal which indicates precisely its position. Lockard & Fozard (103) conclude that a feasible system can be devised in which 68 per cent of the directional data would be in error by less than 3' of arc.

Prosthetic devices.—Terminal devices used by arm amputees are of two types, voluntary opening and voluntary closing. Lore has it that the latter is more "normal." The question is how best to use the unidirectional force of some intact muscle to operate a prehension device, a spring being used to effect the opposing action. Groth & Lyman (71) report that there is no inherent superiority for either type of device. Amputee preference is related more closely to mechanical reliability of the device than to its speed of operation.

FREQUENCY CHARACTERISTICS OF HUMAN RESPONSES

Nearly all of the data on motor responses reported in the preceding sections relate theoretically to the magnitude, or amplitude distribution, of movements or of error. Interest in an orthogonal dimension—the frequency domain—is growing, and the more extensive use of frequency measures is retarded only by the practical difficulties encountered in deriving them.

Fitts *et al.* (54) report the status of a series of studies utilizing autocorrelation and cross correlation records as a means of analyzing frequency characteristics. Among their findings is a consistent tendency of subjects to lead periodic targets (perhaps overcompensating for their own reaction time) in a simple displacement tracking task and to lag aperiodic targets. When they use a pursuit display, the periodicity of the stimulus rapidly disappears from subjects' error records, but with a compensatory display subjects experience continued difficulty in removing these components. The residual random portion of tracking error shows no evidence of peaking at any particular local region.

An alternative approach to frequency analysis is the direct computation of power density spectra. This avenue is used by Sutton (138). He employed a simple task, holding a control motionless against the force exerted by a spring, in which the frequency of the input is zero, i.e., the only stimulus which the subject sees is the feedback of his own error. A local peak of the power spectrum was noted in the region of tremor frequency (9 c.p.s.). Otherwise, in this task subjects exhibit a wide band of error frequencies, with half the total error power between 0.4 and 2.2 c.p.s. There was less variability from one test to another in the higher, or nonvoluntary, frequency ranges.

Several other approaches to the study of frequency characteristics of performance in skilled tasks have been reported. Simon & Smith (134) attempt to count the incidence of short, medium, and long waves in graphic records and discuss a resonance theory of tracking. Hartman (76) uses a graphic record of time on target, which gives data comparable to the time-continuously-on-target score used previously in the Wisconsin laboratory.

Garvey & Mitnick (62) draw an analogy between man's tracking performance and the simplest mechanism which might be substituted to perform the same task in a similar manner. When subjects track a constant rate or a constant acceleration target they lag fast (rate or acceleration) inputs initially, but improve with practice. The authors conclude that at the end of 25 days of practice subjects' performance is analogous to a two-integrator (class II servo) system, i.e., they tend to lag a constant acceleration input by a small but constant amount.

ABSOLUTE JUDGMENTS AND NOVEL ALPHABETS

Tactile communication.—A dramatic example of the application of psychophysical data to the development of an information coding system is provided by Geldard (63). Preceding his "adventure in tactile literacy" were years of work on tactual sensitivity. The immediate question that had to be answered before devising an alpha-numeric code of the sort needed for transmitting normal language messages to the skin, however, concerned the number of levels of different stimulus dimensions that could be identified on an absolute basis with arbitrarily small error. The particular coding system tried in one experiment employed five loci of tactual stimulation (on the

chest), three levels of intensity, and three levels of duration, giving a possible alphabet of 45 discrete symbols. Messages sent to a trained "reader" were accepted at rates substantially higher than the receiving rate of a trained Morse code operator.

Other symbol systems.—Chapanis & Halsey recompute a set of data reported earlier, dealing with absolute color recognition, in order to make their results directly comparable to other recent studies. They conclude that "... if discrete colors are used to code a rectangular continuum, any single value on that continuum can be estimated with an average error of about 2 per cent" (35, p. 102). Using 15 spectrum colors they obtained an estimate of 3.66 bits of information transmitted. The antilogarithm of this number is accepted by some workers as an estimate of the maximum number of categories that can be identified with arbitrarily small error. In this case the number would be 12 categories.

Learner & Alluisi (100), using inclination of a line as the coding dimension, seek a different solution to the formation of a large coding alphabet. They use widely-spaced (45°) steps, and employ variations of an eight-element binary coding scheme (each of eight elements is present or absent), which gives up to 256 possible unique symbols. The utility of such complex symbols hinges, not on absolute discrimination, but on the ability to learn to decode the symbols (make alpha-numeric association to them) rapidly. All of the decoding schemes studied were considerably slower and gave more errors than the reading of conventional alpha-numeric symbols.

Factors affecting absolute discrimination.—Recent studies of absolute discrimination give surprisingly uniform results across a wide variety of stimuli, as Miller (111) points out. The range from five to nine categories covers most of man's abilities to discriminate along simple stimulus dimensions. However, Alluisi (3) calls attention to the fact that the ability to make absolute discriminations is affected by a substantial list of other variables. Eriksen & Hake (53) emphasize the importance of anchor, or end, effects, and Eriksen (52) points out the importance of the frame of reference provided by a series of stimuli. The fact that man has very limited ability for making absolute, in contrast to comparative, judgments and that absolute judgments are susceptible to secondary influences, makes it difficult to devise novel information coding schemes.

Pattern recognition.—Pointing out that visibility data, based upon known shapes presented in known positions, do not apply to tasks in which subjects must search for and identify one of a particular class of objects, Boynton & Bush (23) discuss the need for adequate theory in regard to such things as critical target characteristics and search behavior. Also departing from the traditional research approach, Herrick *et al.* (83) study a simple discrimination task (the two-ness of a radar target) as a function of the characteristics of the background against which the target(s) appear. The background is found to be a much more important factor than is target fading on the phosphor display, or the effect of a radar sweep line.

Rappaport (119) and Fitts *et al.* (55) report on a series of studies of complex pattern recognition conducted with a primary interest in the specification of stimulus pattern characteristics in objective, informational terms. This approach lends itself to the interpretation of results in terms of the effect of signal-to-noise ratio, the band width of signals, pulse repetition rate, and similar parameters of radar, sonar, and other electronic information-gathering devices. Rappaport finds that redundancy of visual patterns is helpful in combating the effects of visual noise, as is the case with other types of messages in noise. Attneave (8) and others, using a more general but slightly less rigorous probabilistic approach, have made other contributions to the specification of the properties of visual patterns.

LEGIBILITY AND INTELLIGIBILITY

Engineering psychologists have often confused legibility and visibility criteria. This distinction is gradually being recognized. Thus, Berger (16) identifies his recent work as being concerned with the "distance threshold of recognition" and Bridgman & Wade, using a visual acuity criterion in determining optimum letter size for a given display area, point out that their results apply when it is desired "... to permit discrimination at maximum distance" (25, p. 380). The authors of the latter study indicate that it is advantageous to dispense with borders and use all available display space for lettering, reasoning that the use of a border, while it improves acuity, does not compensate sufficiently for the fact that less space is available for letters. What the investigators do not check is the finding, reported by earlier workers, that at suprathreshold distances speed of reading is improved by the use of borders and white space between letters, even when this leads to the use of smaller symbols. The issue of whether to design for maximum probability of discrimination at maximum distances and unlimited time, or to design for rapid data processing at nearer distances is an example of the issue of legibility vs. visibility criteria. In this and many other areas, we badly need trade-off functions which will permit a designer to choose a set of design characteristics which is an optimum compromise for his specific problem.

Typographic style.—Optimum typography has been studied for over 50 years and continues to be a subject of interest for psychologists. A comparatively new idea, attributed to T. G. Andrews, has stimulated two studies during the period covered by this review. This is the "square span" idea, in which thought units, phrases which convey a coherent idea, are grouped in a two-line arrangement which may often be encompassed by a single fixation. Nahinsky (113) reports that this more symmetric arrangement yields higher comprehension scores for tachistoscopic exposures, and Klare *et al.* (91) find increased retention scores when the square span is used by superior readers in a normal reading situation, in spite of interference with normal reading habits.

Foley (58) finds results favoring the Lansdell digits, which have an

angular style, over more conventional digits, but fails to emphasize that he is using a visibility criterion. Berger (16), continuing his long-standing interest in typographic problems, reports a series of studies of the effects of grouping and spacing of letters. These effects vary considerably depending on the redundancy of the letter sequences (nonsense vs. meaningful words); optimally spaced words have considerably better distance thresholds than do single letters.

Legibility of scales and instruments.—Chambers (33) reviews and summarizes recent work on various features of scale and dial design, covering such variables as size and shape, method of graduation, types of numerals and pointers, and position of the zero. Adisesiah & Prakash (2), in a field study using Indian Air Force and Navy pilots, find that a one-pointer altimeter, of the type recommended earlier by W. F. Grether, is read more rapidly and accurately; however, errors of greater than 1000 ft. are still made with this as well as with conventional multipointer designs. Older pilots are slower but give more accurate readings.

Baker & Vanderplas (10) varied the number of reference markers (range rings) on a circular display; gross errors and total reading time increased when more reference lines were used, whereas interpolation errors decreased. The use of larger displays decreased the incidence of gross errors, but had little effect on interpolation. Churchill (40) reports a related study of interpolation errors as a function of scale interval length and pointer clearance.

Eye movements play an important role in instrument reading, since in realistic situations instruments are seldom read with less than two fixations (average reading time usually is near a half second or more). Coonan & Klemmer (45) recorded eye fixations for exposure times ranging from 0.1 to 1.0 sec. and related reading accuracy to the number of fixations. The first eye movement, leading to the second fixation, occurs at between 0.15 and 0.30 sec., and there is a sharp drop in errors in this region of the exposure time curve. There is little change in error level prior to 0.15 sec. or after 3.0 sec. The second fixation is almost always made to the pointer, not to the instrument scale.

Auditory displays.—Alternative methods of speech-band compression are discussed by Vilbig & Haase (143) who conclude that the vocoder (voice coding) approach offers greatest promise since it may permit a ten- to twentyfold decrease in the frequency band while maintaining adequate intelligibility. Black (21) investigates the properties of an ear-inset microphone. Bone and tissue conduction through the head give a vibratory signal which can be detected by a transducer placed in the ear canal, and this has several advantages, especially in freeing the area in front of the mouth. However, Black concludes that the ear-inset microphone can compete with a microphone in front of the lips only in an application where it is difficult to use the conventional location.

Special lighting problems.—Kraft (96) reviews the results of a three-year period of operational use of a spectrum-sharing ("broad band blue") light-

ing system, designed especially for the requirements of a large radar control center, where scope observers require a very low ambient light level, but other personnel, such as maintenance workers, need a high level of illumination on a continuing basis. Additional refinements and the theoretical bases for the system are reviewed. Barnard (12) offers a radar room lighting solution that is similar in principle to that applied by Kraft. Helson *et al.* (81) attack the problem of color rendition with fluorescent sources of illumination.

Smith & Boyes (135) recheck the findings of earlier workers on the effects of ambient room illumination on radar screen visibility and find that gains up to 25 per cent are possible if optimum ambient illumination and optimum cathode ray tube bias (background brightness), as compared with the bias levels often used by operators in a dark room, are used. One-tenth ft. c. of ambient illumination is at least as favorable as complete darkness for radar scope observers.

Sights and optical devices.—Hodge (84) reviews tests of modified rifle sights designed for use at low levels of illumination. Harker & Weaver (73) compare ranging performance with two types of stereoscopic reticles and find that the ballistic stereorange finding principle is markedly inferior to a Wandermark range finder. Weiss (145) examines visual acuity and depth perception as a function of magnification.

Campbell *et al.* (32) extend the study of the use of optical periscopes in aircraft which was begun some years ago by S. N. Roscoe. Using a nose-position in a B-17 type aircraft, and 20 pilots, they find satisfactory performance in normal flight but some difficulty in search activities such as are necessary in traffic. They use an optical periscope, whereas Roscoe used an image viewed on a ground glass screen.

Detection, warning devices, check reading.—Gerathewohl (65), who has reported earlier that flashing lights are generally more conspicuous than steady signals of similar brightness contrast, reports a further study dealing with the frequency, duration, and contrast of visual warning signals. At low contrast values, conspicuity increases with flash frequency, up to a limit; with low frequencies, conspicuity increases with brightness contrast. Bartz (13), hypothesizing that dial-checking performance would be improved if a pointer deviation were accompanied by a local change in the color of the dial luminant, finds that this is true with respect both to speed and errors. Kurke (97) proposes a novel type of dial that incorporates both a pointer and a V-shaped sector which exposes differently-colored backgrounds. Use of the colored background reduces check-reading time and errors.

In a search task involving the location of critical numbers in a larger matrix, Green & Anderson (69) discuss the value of color cues in terms of the concept of relevant (redundant) vs. irrelevant information. When subjects know the target color, search time is significantly reduced and is approximately proportional to the number of symbols of that color. Lincoln & Averbach (102) confirm the data of earlier workers on the superiority of

9 o'clock for the normal position when groups of dials must be check-read. In order, from best to worst, for check-reading purposes the sectors of a circular dial are (a) top left, (b) top right, (c) bottom left, (d) bottom right.

Nicely & Miller extend probability learning theory to cover the effect of unequal spatial distribution on the detectability of radar targets. "When targets are much more probable in one area of a radar display than in another there is a decline in sensitivity for the low-probability area and no decline for the high-probability area" (114, p. 198) as the duration of watch increases.

DESIGN FOR SAFETY

This reviewer has not located any significant recent studies, except a few privately published ones, which approach the topic of safety from the viewpoint of equipment design. Jacobs *et al.* (88), however, do discuss engineering aspects of accidents along with mathematical and conventional psychological (selection and training) aspects. A review of engineering psychology would be remiss if the importance of this area were not mentioned, however.

METHODOLOGY IN ENGINEERING PSYCHOLOGY

General methodology.—Chapanis (34) has prepared a review of general research methodology in engineering psychology, with examples from published studies. Although much of the material is available elsewhere, this is the most comprehensive and authoritative treatment now available, and should be especially useful for people who are beginning research in this area. The report is eventually intended to serve as a section in the *Human Engineering Guide to Equipment Design*.

Simulation.—Harter & Fitts (75) outline the steps in analyzing a complex machine system for the purpose of simulating it for engineering psychology research. Fortunately, such an analysis can usually be done as soon as the system is designed and in advance of its construction. Techniques are discussed which permit the construction of general purpose equipment that can be adapted in a matter of hours to the simulation of some new system. The heart of such equipment is a small general purpose computer of the type now commercially available. Hartman & Wetherbee (80) describe a special purpose computer for use in engineering psychology research, and Pinsker (117) discusses the importance of the flight simulator in aircraft control research and design.

Smith and associates report on two ingenious additions to the tools available for the analysis of perceptual-motor performance. An electronic handwriting analyzer (136) gives objective records of movement cycles in writing. The use of a closed loop television camera (137) permits much greater flexibility of manipulation and control over the nature of a displaced visual field than does the older technique of placing lenses before the eyes.

Criterion measures.—A general criticism of the use of opportunistic per-

formance measures, and specifically of the use of arbitrary cut-off points in defining all types of dichotomized performance scores (such as time-on-target and per cent error scores) is offered by Bahrack *et al.* (9). Data from current tracking studies are used to show that the shape of a learning curve is largely determined by the size of the error zone employed in dichotomizing between on and off target. Further data on the relations among various scores of tracking performance are included in the reports by Fitts *et al.* (54) and by Fleishman (56) cited earlier. Bilodeau (20) treats alternative scoring procedures from a viewpoint similar to Bahrack's, but emphasizes the effects of systematic transformation of knowledge of results.

BIBLIOGRAPHIC SOURCES

The Office of Technical Services, U. S. Department of Commerce (149) makes available for public sale most of the unclassified reports issued by Government agencies, and issues a monthly listing of new acquisitions by that agency and by the Library of Congress.

The three military services, acting through the Human Engineering Branch, Office of Naval Research, are supporting a Human Engineering Information and Analysis Service, under the direction of E. V. Saul of the Institute of Applied Experimental Psychology, Tufts College. Bibliographic services are available to individuals or institutions and access to some 9000 documents can be arranged.

The most comprehensive published bibliography which covers the topics included in the present review is that by McCollom & Chapanis (106), which contains 5666 titles, assembled as part of the work of preparing the *Human Engineering Guide to Equipment Design*. Its chief defect at present is the lack of an author index; references are grouped by subject.

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PERSONALITY^{1,2,3}

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The concept of personality is a result of our observation of individual differences in human behavior. Personality research is an attempt to systematize IDs and to account for them in terms of the interaction of constitutional, developmental, and environmental factors. The writer favors a positivistic and behavioristic orientation in this endeavor. The emphasis in this review is more on research findings and on methods and measurement than on theory. The major theories of personality change comparatively little from one year to the next. The minor theories which are conceived after the findings they are intended to explain are ephemeral. Each year brings forth new findings which more often than not embarrass last year's theories. But theories in psychology are seldom disproved; they just fade away. Of course, all present theories of personality are doomed to pass into history. They should be tolerated only in proportion to their heuristic value to research.

Personality research increasingly overlaps general psychology. Differential psychology, measurement theory, learning theory, perception, and physiological psychology are drawn upon more and more in the personality field. Psychoanalysis may appear to be poorly represented in this review. It seems to the writer that as the phenomena that psychoanalytic theory purports to explain come under the purview of psychological research, these phenomena lose their psychoanalytic identity and are absorbed into the more scientifically adequate framework of general psychology.

The writer would like to have included a section on some of the recent developments of methodological significance had space permitted. For example, the study of language behavior, which has already developed a substantial literature within the past few years, should be encouraged for its potential contribution to the understanding of personality. Why responses to verbal questionnaires are related to other aspects of behavior is a central problem in the methodology of this field and is still obscure. The study of the interview and other forms of interpersonal behavior by such means as the Chapple Interaction Chronograph is a promising development. Psycho-

¹ This review covers the period April, 1956, to April, 1957.

² In this chapter the following abbreviations are used: CS (conditioned stimulus); F-scale (California scale for measuring authoritarianism); IDs (individual differences); I-E (introversion-extraversion); MAS (Taylor's Manifest Anxiety Scale); MMPI (Minnesota Multiphasic Personality Inventory); n Ach (need for achievement); n Aff (need for affiliation); P.F. (personality factor); PGR (psychogalvanic response); S (subject); TAT (Thematic Apperception Test).

³ The writer is indebted to Dr. Lowell Storms and Dr. John Sigal for their critical reading of the first draft of this chapter.

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pharmacology—the use of drugs in psychological research—offers a further means of exploring physiological correlates of behavior (29). Since personality research is tending to become an interdisciplinary pursuit, it behoves psychologists to concentrate on developing methods of behavioral measurement and discovering functional relationships between behavior and events in the environment. Without such developments, a liaison with other disciplines such as biochemistry, physiology, genetics, sociology, and anthropology can only be premature and unfruitful.

The reader may note a scarcity of references in this review to studies based on projective techniques. In the writer's judgment the standard projective techniques *qua* projective techniques have been a failure methodologically and substantively in personality research. The one exception seems to be the special adaptation of the TAT by McClelland and his associates in their study of motivation. The Rorschach in particular has been worthless as a research instrument. Though claiming for decades to be the method *par excellence* for studying personality, the Rorschach method has nothing to show for its applications in the personality field. After more than thirty years of research, the vast bulk of Rorschach studies are still attempts to demonstrate some kind of validity of this test. In view of this poor showing, the hopes and claims that continue to be professed (e.g., 20) by the adherents of these methods are indeed cause for wonder.

Two noteworthy books on personality theory have appeared this year. A welcome contribution is *Theories of Personality* by Hall & Lindzey (44), which to date is the only comprehensive textbook on the subject. It presents the theories of Freud, Jung, Adler, Fromm, Horney, Sullivan, Murray, Lewin, Allport, Goldstein, Sheldon, Eysenck, Cattell, Dollard and Miller, Rogers, Murphy, and others. The writer prefers to classify this book as belonging to the literature on the history of psychology, and would recommend its use in courses on the history of psychology rather than as a basic text in courses on personality. In clarity and accuracy of exposition the book is thoroughly first-rate. On the critical side, however, it is weak, but this is due mostly to the fact that the authors' intention was expository rather than critical. The present writer does not share the Hall & Lindzey reverence for theory in general. All of the personality theories are presented in almost equally glowing terms. One wonders what impression of psychology this kind of presentation might make on students who are also taking courses in other disciplines. The appeal is apt to be greater to the humanities students than to those more disposed towards the natural sciences. While Hall & Lindzey liken the function of their book to that of Hilgard's *Theories of Learning*, it should be pointed out that Hilgard's book has the feature, lacking in the Hall & Lindzey presentation, of evaluating each theory in terms of the same criteria of empirical, methodological, and theoretical adequacy.

Perspectives in Personality Theory, edited by David & von Bracken (21), contains contributions by twenty-two European and American psycholo-

gists. The chief value of this book is that it brings us into closer touch with current European thought in the personality field and provides a wealth of bibliographical material for those wishing to delve further. We are given an overview of current personality theories and research in Germany, Switzerland, Britain, Italy, and France. With the exception of Britain, there seems to be little of what we would call scientific research in European personality psychology. Existential psychology, characterology, and stratification theory all seem to be more philosophy than psychology. Much of Continental theory has little contact with data; often it is difficult to know exactly what facts the theory is intended to explain. Psychologists trained in the American behavioristic tradition are likely to find many of the contributions perplexing.

STRUCTURE

Traits and types.—In the early stages of personality research, in which we are sorting out the manifest variety of human behavior so that we can ultimately deal with it on an explanatory level, we must resort largely to ratings and questionnaires and various measures of performance. We know of no better methods than those of factor analysis for "making sense" out of these kinds of data. Factor analysis is most valuable in constructing and selecting tests and in finding principles of classification. Neglect of the dimensional analysis of measures that purport to reflect certain consistent aspects of behavior other than the measures themselves can only lead to the proliferation of innumerable labels and descriptions of various kinds of behavior and the reporting of their correlations with innumerable other measures. If we wish to abandon dimensional analysis and still profess faith in scientific endeavor, we must be content to study by experimental or observational methods the functional relationships between variables which are of primary interest in their own right. This is the procedure in most research on sensation, perception, and learning, and is practically the sole method in physiology. There is much to be said in favor of this approach in the field of personality. However, so long as we use measures (e.g., questionnaires) which are themselves not of primary importance but are merely indirect measures of other forms of behavior, we must be concerned with dimensional analysis. This concern does not necessarily carry over to the applied field, where measures are explicitly constructed for the purpose of predicting a particular criterion. In terms of any concept of personality, what is being measured in such cases is usually unknown, and the reason why the test succeeds in predicting the criterion remains a mystery. Such a state of affairs is utterly unsatisfactory to those whose primary interest is the scientific study of personality.

Research workers are therefore indebted to Guilford and Cattell for their continued efforts to improve their factorial measures of personality. Guilford & Zimmerman (43) broke down the 13-factor Guilford-Martin personality inventories (GAMIN, STDCR, O, Ag, and Co), into 69 clusters each con-

taining anywhere from 2 to 10 items, and factor analyzed the matrix of correlations between these clusters to determine if the original 13 personality factors would emerge. The analysis showed that 14 dimensions were needed to account for the intercorrelations. With certain minor qualifications, 13 of the factors could be interpreted as being nearly the same as those in the original inventories. Interpretations of most of the factors were modified and clarified as a result of the reanalysis. The 14th factor—masculinity-femininity—resulted from including the variable "sex membership" in the intercorrelations. All factors were rotated orthogonally and are independent of one another with minor exceptions.

Cattell (12) has also reanalyzed his Sixteen Personality Factor Questionnaire. Cattell describes the reanalysis as consisting of two aspects: validation, i.e., the determination of the construct validity of the items intended to measure each personality factor; and intensification, i.e., the process of raising the saturation of items on required factors and reducing their correlations with factors other than the intended one. Cattell's "Canons of Factored Test Construction" and his technique of "parcelled factor analysis" are significant methodological contributions. Cattell based his factor analysis on the intercorrelations of several hundreds of individual items. The factor loading of every item of the original 16 P.F. (374 items) was determined and items of low validity were replaced by new items of higher validity. Unlike Guilford's factors, Cattell's are not orthogonal, but have been rotated obliquely to approximate simple structure. In another paper Cattell (13) describes the factor analysis of the matrix of correlations between the 15 primary factors of the 16 P.F. (the 16th factor is one of intelligence and was omitted from the analysis). The analysis yielded four second-order factors which Cattell has labelled (a) anxiety vs. dynamic integration, (b) extraversion vs. introversion, (c) cyclothyme vs. schizothyme constitution, (d) unbroken success vs. frustration. It may be pointed out that Cattell has arrived, by quite different means, at three factors which appear very similar to Eysenck's factors of neuroticism, introversion-extraversion, and psychoticism. The most obvious need at present is a direct comparison by correlational methods of the factors of Guilford, Cattell, and Eysenck. The inventories of Guilford and Cattell, at least, purport to cover nearly all the variance in the personality questionnaire realm. The extent to which the two inventories cover the same territory will have to be determined empirically. Cattell's inventory seems to be supported by more validation material and may have greater generality because of the greater size and diversity of the population samples on which the factor analysis was based.

Factors are of interest to personality theorists in that they serve to define and delimit behavioral phenomena. These phenomena can then be brought under experimental analysis in an attempt to explain the factor on a different level than that of factor analysis itself. This approach is exemplified in the work of Eysenck, who began with large-scale factor analytic

studies using a wide variety of questionnaires, objective behavior tests, and physiological measures in order to discover certain basic and pervasive dimensions of personality. He has amassed evidence for three such (orthogonal) dimensions, which he has labeled introversion-extraversion (I-E), neuroticism, and psychoticism. Eysenck and his co-workers have so far been concentrating on the experimental study of the I-E dimension. Eysenck (25) has hypothesized that the underlying cause of I-E is due to constitutional differences in the capacity for developing and dissipating cortical inhibition. It is postulated that in extraverts cortical inhibition is generated more quickly and strongly and dissipated more slowly than in introverts. In the conditioning and learning field Hull's concept of reactive inhibition is, according to Eysenck, an intervening variable on which introverts and extraverts should differ; and in the perception field Köhler's concept of neural satiation is an hypothetical construct on which introverts and extraverts should differ. Functional similarities between reactive inhibition and neural satiation, suggesting that they are basically the same phenomenon, have been discussed by Duncan (22). Eysenck (25) has attempted to demonstrate that differences in I-E are related to differences in kinaesthetic figural aftereffects, a phenomenon attributed to neural satiation, and also to the rate of acquisition and extinction of conditioned responses (eyeblick), one of the determinants of which is reactive inhibition.

This year Eysenck (26) predicted that in pursuit rotor learning extraverts would show a higher degree of the reminiscence phenomenon than introverts. (Reminiscence was taken as a measure of reactive inhibition.) The outcome of the experiment intended to test this prediction is not very decisive, though the prediction is borne out to a degree. The correlations (based on 50 Ss) between extraversion and two reminiscence measures were .29 and .10; only the first is statistically significant. A repetition of the experiment in Eysenck's laboratory by Star,⁶ using 100 Ss, produced a correlation of .17 ($p < .05$, one-tailed test) between I-E and reminiscence. The correlations between two reminiscence scores and neuroticism in Eysenck's experiment were .40 and .27, which are significant. This correlation between neuroticism and reminiscence, which was not predicted, is interpreted by Eysenck as follows (26, p. 332).

If one regards neuroticism as a drive, and if drive leads to an increase in the amount of reactive inhibition tolerated by the organism before producing a cessation of activity, then individuals with strong drives should produce greater amounts of reactive inhibition and consequently higher reminiscence scores.

Repeating the experiment on 100 Ss, Star obtained a negative correlation of $-.24$ ($p < .05$) between reminiscence and neuroticism. Since the neuroticism and I-E measures correlate only .115 (not significant) it seems justifiable to interpret the correlations of both I-E and neuroticism with reminiscence as due to the interaction of two different factors, viz., drive and

⁶ Personal communication, June, 1957.

reactive inhibition. It is also possible to argue that differences between introverts and extraverts in pursuit rotor performance, including the reminiscence effect, are due to more superficial differences than cortical inhibition. That is, the I-E questionnaire may differentiate people in terms of conscientiousness and willingness to comply with the experimenter's instructions, and these tendencies would be reflected in the S's performance. Eysenck makes it clear that he would not agree with such an interpretation (26, p. 331):

It would not be correct to say that personality factors, such as neuroticism and extraversion, *determine* the degree of reminiscence shown. Both reminiscence and personality factors are conceived of in this theory as determined by more fundamental causes, such as the inhibition/excitation balance, or the amount of autonomic drive present in a person.

Franks (37), working in Eysenck's laboratory, reports further evidence for differences between introverts and extraverts in rate of acquisition and extinction of the conditioned eyeblink reflex. In a previous experiment (35) neurotic patients were used; the present experiment used 60 university students. The findings of the two studies are almost identical. The correlation between extraversion (E) scores and speed of conditioning was $-.46$ and between E scores and slowness of extinction $-.34$. At least two criticisms of Franks' experiments may be made. No evidence is presented that I-E is not related to rate of eyeblink⁶ or to sensitivity to the air-puff independently of the conditioning. Also Franks' conclusion that I-E is related to conditionability is not fully warranted since he has used only one measure of conditioning, viz., eyeblink. No general factor of conditionability has been demonstrated. However, a project is now under way in Eysenck's laboratory to determine the existence of a general conditionability factor, based on an analysis of eyeblink, salivary, sensory, PGR, and EEG alpha rhythm conditioning.

Eysenck has hopes of ultimately working out the genetic aspect of his personality factors. The only evidence he has presented for the inheritance of I-E (27) is not convincing. The correlation between I-E factor scores was found to be higher for identical than for fraternal twins. But the factor analysis on which these scores were based must have been somewhat disconcerting to Eysenck. Only one of the tests used by Eysenck to measure I-E in other studies was used in this study (an adaptation of Guilford's R scale) and it had an insignificant loading of $-.09$ on the I-E factor. The most heavily loaded (.63) variable on the I-E factor was the Rorschach M per cent. If Eysenck puts much stock in the conclusions he draws from this study, one may wonder if he might abandon the I-E questionnaire which he has used in many experiments and use the Rorschach instead. There seems to be little danger of this happening.

The neuroticism and psychoticism dimensions have received attention

⁶ Franks has discussed this point at length in his unpublished Ph.D. dissertation (see 35).

from S. B. G. Eysenck (32), who tested groups of neurotics, psychotics, and normals on a number of objective tests and, by means of a discriminant function analysis, showed that two dimensions are needed to describe the differences between neurotics, psychotics, and normals. Her study thus lends further support to H. J. Eysenck's conclusion, based on earlier work, that neurosis and psychosis do not represent merely varying degrees of maladjustment along a single continuum but must be conceived as two different dimensions.

Lorr & Rubinstein (62) factor analyzed the correlations between ratings on fifty variables made on a group of neurotic patients by their psychotherapists. Ten primary factors emerged and two orthogonal second-order factors. The first was identified as the type of personality disorganization that occurs when ego defenses crumble; the second factor represents a tight defense system with extropunitive elements.

Singer *et al.* (82) factor analyzed a number of measures of ego functions such as delaying capacity, fantasy tendencies, level of aspiration, planning ability, motor inhibition, and responsiveness in ward behavior. The battery was administered to 100 male schizophrenics. Factor analysis yielded four factors labeled (a) motor inhibition and planfulness, (b) ambitiousness or need achievement, (c) emotional surgency, (d) introspectiveness or introversiveness. Two second-order factors emerged, one linking emotional surgency and lack of introversiveness, the other linking motor inhibition and ambitiousness.

Jones & Morris (53) factor analyzed Thurstone's Temperament Schedule along with a scale measuring values and "philosophy of life." The measures of temperament and of values rarely have more than 10 per cent of their variance in common; much further investigation would be needed before even this fact could be meaningfully interpreted. All three of the above-mentioned studies would have yielded more valuable information from an integrative point of view if "reference" inventories such as those of Guilford or Cattell had been included in the factor analysis.

Rigidity studies are definitely on the wane, most likely because no one has succeeded in demonstrating a general trait of rigidity or in getting various measures of rigidity to intercorrelate significantly among themselves, let alone with anything else. The literature on the water-jar Einstellung test, the favorite measure of rigidity for almost a decade, has been reviewed by Levitt (59), who concluded: "1. After eight years of research, evidence for the validity of the water-jar test as a measure of rigidity is still lacking. 2. The water-jar test is a poor psychological test *qua* test" (59, p. 368).

Fundamental processes.—Perception and learning have rightfully held the center of the stage in psychology for half a century. It is in these fields that psychology has achieved the most substantial and systematic body of knowledge and the most highly developed and satisfactory theories. Here the discovery of general laws has always been the chief aim. In experiments on perception and learning, IDs are regarded as error variance, and person-

ality factors, if they are noted at all, are thought of merely as interfering effects that make the discovery and demonstration of general laws more difficult. It is in just these factors, so long neglected in their own right, that personality research may find some of its most fruitful subject matter. Nowadays few psychologists would wish to identify personality so narrowly as only those aspects of behavior which can be described in terms of questionnaire responses, ratings, projective techniques, or clinical observation. All IDs in the behavioral realm may be regarded as the subject matter of personality research. Perception and learning are perhaps the most fundamental behavioral processes and are the basis of much that we call personality. This is especially true of learning. The systematic investigation of IDs in these processes, as well as of their developmental aspects, is therefore highly germane to personality study. We have the advantage here of entering not a wilderness but charted territory. Years of research have provided us with much substantial knowledge of the phenomena of perception and learning; we know something of the parameters involved, of important functional relationships, of general principles and laws. Not the least important from our point of view is that we are already provided with many experimental techniques for the study of these processes.

Personality researchers have hardly begun to cultivate this territory. True, it is commonly heard that personality factors (or emotional factors) affect learning and perception, as if personality were another part of the individual exerting an influence on certain more fundamental processes. That personality is a product of learning is also a widely held view and is often accompanied by an implicit belief that persons differ from one another in the content or effects of their learning, while the learning process itself is the same for everyone. It is granted that the influence of past experience on perception and the learned aspects of behavior are important subjects for study. They are not being overlooked. But the study of IDs in the processes themselves has not yet come into its own. How can one best proceed in this investigation? Should one start by looking for correlations between personality factors derived from questionnaires and ratings on the one hand and measures of perception and learning on the other? Or is this too wide a gap to be bridged meaningfully by a correlation coefficient? Should one look first for factors, or traits (primary factors) and types (second-order factors), strictly in the realm of perception or learning, and then try to find links with the personality factors derived from other measures? Surely many such questions must be asked. But at the present stage we had best encourage every approach that appears at all promising.

Perception.—We are reminded by Granger (41) that from the psychophysiological point of view the peripheral sense receptor in the case of vision is not only a sense organ in the strict sense of that term, but is also a part of the central nervous system. Visual as well as perceptual phenomena may therefore provide a means of approach to the study of brain functioning. Granger cautions, however, that in experiments on IDs in perception, ef-

fects on the sensory level should be excluded or accounted for before postulating differences at a higher level (41, p. 72).

The year's major effort in the perception field is reported in a monograph by Eysenck, Granger & Brengelmann (30). The experimental work on which this monograph is based was completed three or four years ago and is not to be regarded within the framework of Eysenck's present theory of I-E. Furthermore, it is best viewed as a large-scale exploratory study. Forty-three different sensory and perceptual tests, along with a number of cognitive, motor, physiological, and verbal personality measures were obtained on large groups of neurotics, psychotics, and normals. Granger reports the findings on simple sensory and peripheral processes—dark vision, visual acuity, accommodation, color vision, critical flicker frequency, autokinetic effect, etc. Brengelmann describes the results from a number of perceptual techniques which have been developed in Germany but are little known abroad; these represent more complex perceptual processes such as illusions, afterimages, vision through prismatic lenses, perceptual learning, etc. A centroid factor analysis of 77 scores derived from all of the tests is discussed by Eysenck. The results of this analysis are hard to evaluate. The first two factors are defined by the verbal personality measures and the intelligence tests; the third and fourth factors have their highest loadings on the perceptual tests but are not easily defined. The communalities of the various measures are quite small and in the majority of the tests the factors do not account for any reasonable portion of the communality. How much this may be due to specific factors or to error it is impossible to say since the intercorrelations were not corrected for attenuation. In general, however, the results of this study leave no doubt that perceptual processes are somehow related to more molar personality variables, particularly to type and degree of mental illness. Many more of the tests discriminated significantly between normals, neurotics, and psychotics than could be expected by chance. The verbal personality tests of neuroticism generally differentiated the groups more effectively than the perceptual tests, a not surprising fact, since the personality tests were explicitly constructed for this purpose. Physiological tests (salivary output, temperature, pulse rate, blood pressure, PGR) showed no marked differentiation between diagnostic groups, while motor responses (body-sway, dexterity, Luria test) differentiated somewhat less effectively than the perceptual tests. Eysenck points out that the facts revealed in this study cannot be accounted for by any existing theory of personality (30, p. 128):

Most existing theories may be adequate to deal with highly abstract concepts having little or no anchorage in behavior, but they do not enable us to make predictions in the field covered in this monograph. . . . Scientific theories cannot choose the facts that they would wish to explain in any given field of study. If a theory fails to account for plainly relevant facts, this is a serious argument against it. The reader may like to try to explain the phenomena described here [i.e., the relationship between perception and personality disorder] in terms of archetypes, oedipus complexes, life styles, or any other explanatory concepts used by "dynamic" schools.

The "personality through perception" approach of Witkin, which attracted a good deal of attention a few years ago, appears either to be lying dormant or to have died out. It is given attention in only two papers this year (36, 42). Witkin's work represents one of the first concerted attempts to relate perception to personality. The relationship between mode of perception and personality was conceptualized in terms of a person's tendency toward active coping or passive submission with respect to the environment. The Rod-and-Frame Test and the Tilting-Room-Tilting-Chair Test were the principal techniques used by Witkin. This research was methodologically weak and partly for this reason has failed to command the interest of other personality researchers. Gruen (42) presents a thorough critique of this work. Franks (36) failed to find that Witkin's Rod-and-Frame Test correlates with introversion-extraversion, neuroticism, or with the effects produced by a depressant drug (sodium amytal).

We are indebted to Jenkin (52) for his comprehensive and critical evaluation of the literature on affective processes in perception. It is now reasonably well established that certain motivational states are determinants of size judgment; need is a determinant of perception; there is selective sensitization for stimuli presumed to be noxious or threatening to Ss (perceptual defense). Jenkin points out that in designing experiments on perceptual defense it is necessary to take account of certain critical IDs. An interesting illustration of this is seen in a study by Carpenter *et al.* (10) in which it was found that Ss judged as being repressors on a sentence completion test perceived conflict words more slowly than did Ss judged as being nonrepressors or sensitizers. Spence (87) also found that certain Ss had lower recognition thresholds for threatening words than for neutral words (vigilance) while others had higher thresholds for threatening words (defense). The absolute value of the discrepancy between thresholds for threatening and neutral words correlated positively with degree of anxiety.

A novel experiment by Smith & Raygor (84) relates word association to the concept of visual satiation and demonstrates a correlation between satiation and personality. Prolonged visual exposure of a stimulus word resulted in a word-association response less common (in terms of the Kent-Rosanoff frequencies) than that elicited under brief exposure. Ss characterized by a personality inventory as sensitive, flexible, imaginative, and extraverted differed in this satiation effect from Ss characterized as rigid, withdrawn, and introverted. Presumably there is a hierarchy of potential responses to a given stimulus word, and with continued exposure to the stimulus word each response in order will be aroused and then become refractory or satiated, so that in time distantly related response words will occur.

Learning.—The writer believes that Skinner's method of analyzing behavior in terms of operant conditioning promises to become a most important contribution to personality research. As applied to the study of personality, Skinnerian methods, too, are just at the beginning. A few years

ago Skinner, Lindsley, Solomon, and their associates at Harvard began studying psychotic behavior by putting schizophrenic patients into human-size Skinner boxes. Skinner's (83) ideas about the study of psychotic behavior are completely consistent with his approach to the study of animal behavior over the past thirty years. Recent reports (60, 61) of the research on psychotics reveal the exploratory nature of this work so far, but they also indicate the exciting possibilities of Skinner's methods. Rate of responding under operant conditioning is related to severity of psychosis; King *et al.* (56) have found this relationship to be curvilinear. There are marked IDs in the effectiveness of various reinforcers (61). The various reinforcers used in the Harvard studies were candy, nickels, food, cigarettes, pictures, and music. Also the S's bar pressing was made instrumental in feeding a hungry kitten, the sight of which by the S acts as a reinforcement. The effect of various drugs on operant behavior is also under investigation.

Conditioning of verbal behavior has been the subject of several studies this year. They suggest that we have here, at least potentially, a means of approach to the understanding of attitude formation, the self concept, interests, defense mechanisms, and questionnaire behavior. McNair (66) has shown that a S's rate of responding verbally to pictures could be markedly influenced by various rates of reinforcement and that this process could take place without the S's awareness. The amount of talk about particular aspects of the pictures was also influenced by reinforcements. Nuthmann (70) found that the experimenter's saying "good" served as a reinforcement for the conditioning of acceptance of self on a personality questionnaire regarding self attitudes. A nonverbal stimulus light was not an effective reinforcer in this situation. This learning, too, came about without the S's awareness. Hildum & Brown (48) administered a questionnaire by telephone and were able to bias the S's responses through the selective interpolation of "good." "Mm-hmm" was not effective. In an ingenious experiment Eriksen & Kuehe (23) demonstrate avoidance conditioning without awareness as an analogue of repression. In order to explain repression in conditioning terms it is necessary to show that implicit verbal behavior or thoughts are analyzable into S-R sequences and that these S-R sequences are learned and modified by the same principles of reinforcement that govern other behavior. In repression it is also necessary that the anxiety-provoking thought be prevented from occurring. This process is automatic, occurring without the S's awareness. Eriksen & Kuehe's experimental analogue meets these criteria of repression. After certain arbitrarily-selected words among the S's chain associations to a word-association test were punished by an electric shock, these words were repressed in the S's later trials on the same word association test even when there was no threat of shock.

Autonomic and psychomotor responses.—The investigation of autonomic functions, which hold a prominent place in certain theories of emotion and of neuroticism, has so far not proved fruitful in personality research. Autonomic responsiveness as presently measured does not show reliable systema-

tic relationships with other psychological variables. The highly specific and relatively autonomous nature of the various autonomic functions is probably responsible to a large degree for the failure to find consistent relationships between autonomic activity and personality variables. From the literature on PGR as well as from her own experimental studies of PGR, S. B. G. Eysenck (31) has concluded that little progress is likely to be made in working with the PGR until fundamental problems of measurement are resolved. These difficult and complex problems are being ably tackled by Lacey (58).

In the psychomotor realm even such seemingly simple types of behavior as reaction time and performance on simple repetitive motor tasks have been used effectively by Venables & Tizard (90, 91) as a means of testing hypotheses deduced from Pavlov's little-known theory of schizophrenia.

Attitudes.—One of the major preoccupations in the personality field is that of bridging the gap between psychology and sociology. Thus a great deal of recent theory and research is concerned with the integration of basic psychological phenomena with socially and politically relevant behavior. The focus of these efforts is the study of attitudes. In this field the research on authoritarianism continues to hold the center of the stage. Surveying the great amount of work that has been done in this area since the publication of *The Authoritarian Personality* (1) provokes one generalization: the formula for creating a research craze of proliferation and longevity consists of making available an easy-to-use measuring device with a significant label and fascinating content. Factorially it should be as multidimensional as possible, so that it will yield significant correlations with a host of other psychological measures.

Such has been the case with the questionnaires of authoritarian attitudes, particularly the well-known F scale. In past years ethnic prejudice and authoritarianism, as measured by the A-S (anti-Semitism), E (ethnocentrism), and F scales, have been shown to be significantly related to rigidity, concreteness, narrowness of thinking and problem solving, premature closure of perception, intolerance of ambiguity, distortion of memory, intelligence, xenophobia, family ideology, anxiety, reinlistment intent, cooperation in experimentation, and leadership qualities, to name only a few of the variables investigated. The major findings with the F scale from 1950 to 1955 have been reviewed by Titus & Hollander (89). In view of the many unanswered questions concerning the F scale, they caution against its use as a practical instrument in applied settings. The present writer would go a step further and say that even its use in personality and social research should be questioned, except for direct investigations of the scale itself. It is unfortunate that this sort of analysis was not undertaken several years ago, before the accumulation of so many findings that are now practically impossible to interpret. Much of the research on authoritarianism has consisted of correlating one unknown with another. It is now clear that the F scale is multidimensional and all but a minor portion of its variance can be attributed to a number of factors that have little to do with the content

validity of the scale. Factors of intelligence, educational level, response-set (acquiescence), general maladjustment or neuroticism, and probably a social-desirability response factor are involved.

This year for the first time since the publication of the F scale in 1950 we find that an appreciable proportion of the studies in this area have dealt with the nature of the F scale itself. The findings provide a striking demonstration of the importance of dimensional analysis of personality and attitude questionnaires. All of the F scale items are stated positively, so that agreement represents authoritarianism. A number of investigators have devised reverse F scales in which the same item content is worded negatively. When the positive and negative F scale items are combined into one scale, the paradoxical property of negative reliability may be obtained. That is, some Ss agree with pairs of items both of which are mutually contradictory. A response-set of acquiescence, a tendency to agree with generalized statements, is held responsible for this phenomenon. Chapman & Campbell (14) found correlations between individual positive F scale items (F+) and reverse items (F-) to average close to zero. All of the variance in F cannot be due to response-set, however, or the correlations between F+ and F- items would have been much higher. It turns out that the acquiescence response-set is correlated positively with the item content of F. Independent measures of response set and item content correlated .32.

Different investigators usually have quite different ideas as to what constitutes a reversal of any particular F scale item. Obviously there are many ways in which the same content may be worded. A worthwhile investigation would be to factor analyze the items of half a dozen or more reverse F scales and compare the factor loadings of the different wordings of the same item content. It is not unlikely that a larger proportion of the variance will be attributable to the structure rather than to the content of the items.

Jackson & Messick (50), using another reverse F scale, found a correlation of +.03 with the E (ethnocentrism) scale, whereas a negative correlation would be expected on the basis of the item content. In the same study, Gough's *Pr* (prejudice or intolerance) scale of the MMPI correlated +.23 with the reversed F scale, a correlation nearly as high as that between *Pr* and the positive F scale! Since 29 of the 32 items in the *Pr* scale are keyed "true" for intolerance, response-set would seem to account for most of the variance *Pr* has in common with authoritarian measures. Cohn (19) reports a significant correlation of .41 between the F scale and a tendency to answer "true" to a specially constructed scale of 33 MMPI items which discriminated between high and low true responders on the MMPI. Some indication of the confounding effects that the response-set factor can have on the interpretation of correlations is shown in the finding of Jackson *et al.* (51) that rigidity, as measured by the Einstellung water-jar problems, is correlated with F, but only by virtue of the response-set factor rather than the content validity of the scale.

The same problem arises when the F scale is correlated with other multi-dimensional scales, such as those of the MMPI. The interpretation of such correlations can hardly be attempted. The psychopathology of authoritarianism remains one of the important controversial problems in this field; unfortunately it has not yet been investigated by adequate techniques. The principal instrument in most investigations has been the MMPI, and it, too, is afflicted by response-sets (5). Studies of this type should at least include a number of measures of response-set (6, 19, 34) which can then be partialled out of the correlations between the MMPI and authoritarianism scales.

There are many instances in which the F scale continues to show significant correlations with a variety of external, nonquestionnaire criteria which appear to be related to the dynamic concept of the authoritarian syndrome originally formulated in *The Authoritarian Personality*. The conclusion of Freedman *et al.* (38), based on the negative correlation between F and the Hy scale of the MMPI, that repressive tendency is negatively related to authoritarianism, is directly contradicted by an experimental study by Kogan (57) in which low F Ss showed greater recognition of aggressive and sexual statements presented on a tape recorder masked by a noise background. The "authoritarians" showed a greater amount of perceptual defence against sexual and aggressive material, a finding that is consistent with the hypothesis that repressive tendency is a dynamic component of the authoritarian syndrome.

A kind of validation study of the F scale was performed by Wells, Chiavallo & Goldman (92). They asked five college fraternities to fill out a "Guess-Who" questionnaire made up of items reflecting authoritarian characteristics. The fraternities differed significantly on this authoritarian-nonauthoritarian reputation continuum; and they differed in their mean F scale scores, the order of the means being the same as the order on the "Guess-Who" continuum.

The most important contribution to attitude research during the current year is the work of Rokeach (74) and Rokeach & Fruchter (75). Rokeach has argued that the original measures of authoritarianism have to do with right-wing or conservative, rather than with general, authoritarianism and intolerance. He has therefore devised a Dogmatism scale which embraces general authoritarianism and general intolerance regardless of specific ideological content. Rokeach defines the concept of dogmatism as (72)

- (a) a relatively closed cognitive organization of beliefs and disbeliefs about reality,
- (b) organized around a central set of beliefs about absolute authority which, in turn,
- (c) provides a framework for patterns of intolerance and qualified tolerance towards others.

Another questionnaire, the Opinionation scale, yields measures of right and left opinionation. With Rokeach's scales it is possible to reconcile some of the seemingly contradictory or paradoxical findings obtained with the old auth-

oritarian scales, such as the fact that some low authoritarians score high on ethnocentrism and vice versa. Rokeach doubts that the F scale represents an ordinal continuum; there is evidence that it actually measures at least three distinct sorts of authoritarianism. In a number of studies using samples from a variety of populations, the theoretical claims made for Rokeach's Dogmatism and Opinionation scales appear to be well supported (74). The Dogmatism scale correlates highly with the F scale but also correlates positively with both left and right opinionation. A factor analysis of scales measuring ten variables—anxiety, paranoia, self-rejection, dogmatism, authoritarianism, rigidity, ethnocentrism, liberalism-conservatism, left opinionation, and right opinionation—revealed that (a) dogmatism is factorially discriminable from authoritarianism; (b) that dogmatism, paranoia, self-rejection, and anxiety are factorially similar (75).

Most important theoretically is Rokeach's paper on the relationship between belief, as measured by the Dogmatism scale, and thought, as measured by cognitive tasks (73). The concepts used in describing the properties of belief-disbelief systems have much in common with the properties of certain kinds of problem solving, a fact which permits Rokeach to develop a theoretical model subsuming belief systems and cognitive functions.

Another promising attempt at a theoretical integration of attitude research with more general psychological principles is that of Helson *et al.* (47). They have applied the theory of adaptation-level, which has previously related a variety of phenomena in psychophysics, judgment, and perception, to the study of attitudes. According to this theory, adjustive behavior, including the expression of attitudes, is determined by three sources of variance: stimuli immediately confronting the individual, background stimuli, and residual effects of stimuli from past experience. The operation of these factors is demonstrated in a well-designed experiment in which Ss responded to a scale of attitudes under simulated group conditions and alone. Some tentative evidence for the effects of more endogenous factors than past learning is presented by Winthrop (93), who compared various Sheldon somatotypes on a scale measuring consistency of attitudes. Ectomorphs were the most consistent, endomorphs the least, with mesomorphs in an intermediate position. The fact that the Ss somatotyped each other and were all students in a course on Sheldon's constitutional psychology introduces an unknown quantity into this experiment.

One of the boldest attempts in recent years to formulate an integrated theory of personality and social and political attitudes, Eysenck's *The Psychology of Politics* (24), gave rise to one of the most aggressive exchanges of criticism and rebuttal ever to appear in the *Psychological Bulletin* (15, 16, 28). Christie's painstakingly thorough critique of certain aspects of *The Psychology of Politics* is concerned mainly with what he considers defects in Eysenck's methodology and the invalidity of the conclusions based on these methods. Eysenck thanked his critics (also 76) for turning up a few minor misprints in his book, but did not agree with any of the major criticisms.

Motivation.—Motivation is today probably the liveliest and most vigorous, if not the most mature, area of psychological research. The current year's publications testify to this generally good state of health. Contributing strongly to this impression is the fourth annual *Nebraska Symposium on Motivation* (7), containing papers by Beach, Koch, Marx, Miller & Swanson, Seward, and Solomon & Brush. The papers are extremely diverse in their approach to motivation. Those of Beach and Solomon & Brush, based mostly on animal studies, are solidly empirical; theory, what little there is of it, is kept scrupulously close to the facts. At the other extreme are the papers by Seward and Koch. Seward presents a stimulating but highly speculative neurological model for motivation, and Koch intimately expounds on the dim view he takes of the contribution of animal research to the understanding of human motivation. He is concerned about what he considers the inadequacy of current theoretical approaches for dealing with "intrinsically" motivated behavior, such as creative and aesthetic experience. Beach summarizes his extensive research on masculine sex drive, which he conceives as an appetite rather than as a drive in the same sense that hunger and thirst are drives. Appetite is a product of experience, and to a much greater extent than is true of hunger or thirst, sexual behavior depends upon external stimuli and learned cues for its arousal. Marx presents an experimental and theoretical analysis of the relations between frustration and drive. Miller & Swanson describe a system for classifying the variables involved in the resolution of inner conflict; they include needs, morals, defenses, and expressive styles; these are related to certain social class variables. The excellent paper by Solomon & Brush on anxiety and aversion is referred to later in this review.

Motivation research based on fantasy measures of motive strength has flourished this year, and since this line was not given attention in last year's *Annual Review*, a fairly complete summarization of the present state of this research will be attempted here. So far most of the work has concentrated on the achievement motive and the affiliation motive. When this approach to the study of motivation began a few years ago with the work of McClelland and his associates, the theory, methodology, and experimental results seemed relatively simple and straightforward. But as new experiments have been performed and new facts discovered, the picture has become increasingly complex, and the multiplication of *ad hoc* hypotheses now characterizes the theorizing in this area. At present the important thing would seem to be to acquire an adequate body of reliable experimental findings. Fortunately, the methodology of this research is becoming more sophisticated and there seems to be little cause for concern that the workers in this field are not equal to the problem of dealing adequately with the complexity of their phenomena.

Little attempt will be made here to discuss the theoretical issues involved. No theory is at present anywhere near adequate to embrace all of

the diverse facts. For example, how can one even begin theoretically to relate the observations (a) that Ss with high *n Ach* show greater preference on the Strong Vocational Interest Blank for occupations involving financial risk than Ss with low *n Ach* (64), and (b) that Ss with high *n Ach* prefer the colors blue and green over red and yellow, while this is not true of Ss with low *n Ach*? (65). The induction of a theoretical framework will become possible after further substantiation of the findings of many one-shot experiments, along with the systematic investigation of what seem to be the important variables.

Prior to the year covered by this review, the main facts concerning *n Ach* were the following: *n Ach* showed a relationship to college grades, speed of learning, output in performance tests (e.g., anagrams and scrambled words), recognition thresholds for tachistoscopically presented achievement-related words, and ability to recall incompleted tasks (Zeigarnik effect). But these relationships are by no means simple. For example, *n Ach* has been found to be related to ability to recall incompleted tasks only when the tasks were presented to the Ss under the guise of being tests of important abilities. The relationship of *n Ach* to performance is apparently a function of situational factors as well as of internal motives. The relationship depends on whether or not the S performs in a neutral or in an achievement-oriented situation. It is at this point that expectancy theory enters the picture. The simplest theoretical formulation is $B = f(m, e)$: the strength of a behavioral tendency is a joint function of the strength of a particular motive (e.g., *n Ach*) and the strength of the expectancy that a particular act is instrumental to attainment of the goal of that motive (e.g., a task given to Ss in a competitive or otherwise achievement-oriented setting). This conception has considerable support from a number of studies showing that when an incentive unrelated to *n Ach* is offered, no systematic relationship is found between performance and *n Ach*. The same also has been found true of the affiliation motive (*n Aff*).

In summarizing this year's contributions it is impossible to describe the features that are peculiar to each experiment. *N Ach* and *n Aff* are measured by a variety of techniques (the most common being the TAT or other specially selected pictures), and the method of creating neutral and motivating performance situations differs from one experiment to another. For these details the reader must be referred to the original articles.

Hurley (49) found a positive relationship between *n Ach* and rate of learning (nonsense syllables) when learning took place under neutral (low motivating) conditions, but not when Ss received instructions intended to create high motivation. This finding is not as typical as the reverse: that *n Ach* is correlated with performance only when performance takes place under achievement-oriented conditions.

Karolchuck & Worell (55) found no correlation between *n Ach* and learning, although there was a positive correlation with incidental learning. Because no information is given about the motivating conditions under which

learning took place and since the learning task was not at all commensurate with those of other experiments, these results cannot be interpreted. There are a number of instances in which *n* Ach was positively related to performance only when the performance took place under achievement motivating conditions.

Atkinson & Raphelson (2) found that *n* Ach was reflected in various indices of persistence in task performance and in the recall of interrupted tasks only when the Ss had been led to believe that performance was a measure of personal accomplishment. When instructions for performance were designed to minimize this expectancy, there was no systematic relationship between *n* Ach and behavior. When the situational context was such as to minimize achievement motivation and increase motivation to please the experimenter by being cooperative, etc., there was no correlation between *n* Ach and recall of interrupted tasks; but there was a positive correlation between *n* Aff and recall.

French (39) has demonstrated other behavioral correlates of *n* Ach and *n* Aff. She found that the behavior of a person making a choice between a work partner who was a competent non-friend and one who was a less competent friend can be predicted from the relative strength of the person's *n* Ach and *n* Aff. Ss high in *n* Ach and low in *n* Aff chose the competent non-friend, while Ss high in *n* Aff and low in *n* Ach more often chose the less competent friend.

Another factor in the relationship between motivation and performance is pointed out in a study by Atkinson & Reitman (3). *N* Ach was positively correlated with performance (making Xs in circles) only when the expectancy that performance is instrumental in producing a feeling of pride was aroused and few if any other expectancies of goal attainment were aroused. When motives for other goals were also aroused (affiliation and money) by manipulation of situational cues that activate the S's expectancies, there was no relationship between *n* Ach and performance. Clark *et al.* (18) have demonstrated that *n* Ach has two aspects—hope of success (HS) and fear of failure (FF). A level of aspiration questionnaire related to grades in a college examination was used to obtain measures of the HS-FF continuum. The relationship between *n* Ach scores and the HS-FF continuum proved to be quite complex rather than linear. Ss at the extremes of the HS-FF continuum had lower *n* Ach scores than Ss in the middle of the continuum. However, when the *n* Ach score was broken up into two components, one subscore consisting of positive goal imagery (an approach motive with anticipation of reward) and the other subscore consisting of deprivation imagery (an avoidance motive involving anticipation of punishment), it was found that Ss at the extremes of the HS-FF continuum had higher positive *n* Ach subscores than Ss in the middle of the continuum; Ss in the middle of the HS-FF continuum had higher negative *n* Ach subscores. Further complexities of the relation between *n* Ach and performance measures are revealed in a factor analytic study by Clark & McClelland (17), in which anagrams were

the performance task. A factor analysis of anagrams scores and TAT *n* Ach scores resulted in three factors, one of which increased markedly from neutral to achievement-oriented test conditions. This factor in the anagrams test was interpreted as representing an achievement drive. The TAT measure of *n* Ach, however, did not correlate with this factor, though it did correlate with another factor which decreased from neutral to achievement oriented conditions. These findings are hard to interpret and at present only rather sketchy *ad hoc* hypotheses can be put forward. Attempts at theorizing had better await further substantiation of the findings.

Martire (67) reports that Ss with high *n* Ach showed a greater self-ideal discrepancy in ratings of five achievement-related traits (intelligence, initiative, creativeness, motivation, general success) than Ss with low *n* Ach. Miller & Worchel (68), also studying *n* Ach and self-ideal discrepancy, did not find any such relationship.

Somewhat more remote from the central core of this research are the latest findings of McClelland, that Ss with high *n* Ach have a preference on the Strong Vocational Interest Blank for occupations involving financial risk; contrary to what one might expect, there is no evidence that Ss with high *n* Ach prefer occupations of highest prestige value in society (64). It was also found that children with high *n* Ach take only moderate risks (determined by a ring-tossing game), while those with low *n* Ach played very safe or took excessive risks (65).

Taking a lead from the finding that Ss with high *n* Ach have a lower recognition threshold for achievement-related words, Atkinson & Walker (4) investigated a similar phenomenon with respect to the affiliation motive. Since work recognition thresholds are a function of many factors such as word frequency, the S's verbal habits, etc., Atkinson & Walker used pictures, rather than words, presented tachistoscopically under low illumination. They report a positive relationship between *n* Aff and perception of pictures of faces presented below the threshold of conscious recognition. French & Chadwick (40) found that while *n* Aff is not related to social popularity, Ss with high *n* Aff estimate their popularity level more accurately and estimate it as higher than Ss with low *n* Aff.

Anxiety.—The subject of anxiety has inspired a great deal of research in recent years and continues to do so. The reason is not hard to find. Anxiety is a central concept in the field of personality. Few, if any, other concepts sustain so much of the superstructure of personality theory. Anxiety may be most simply regarded as an emotional response conditioned to previously neutral stimuli which have been associated with pain or noxious stimulation. This emotional response, anxiety, is often associated with changes in performance. Anxiety gains theoretical importance when it is conceived as a state having the reinforcing and energizing properties of a drive. The reduction of this drive is held to be the reinforcement for much of social learning; it is the basis for perceptual defense and for all the defense mechanisms in psychoanalytic theory that are attributed major importance

in the development of the personality. Neurotic symptoms, too, are said to be maintained because they afford a degree of escape from anxiety.

The literature on anxiety is more pervaded with theoretical formulations than almost any other aspect of personality research. The multifarious research findings are confusing and contradictory. A few orienting remarks would therefore seem to be in order before reviewing the year's contributions.

On the theoretical side, anxiety has quite generally come to be regarded as a drive state having the properties attributed to drive in Hullian learning theory. Thus any behavior associated with the reduction of drive is reinforced, and the probability, speed, and strength of its occurrence, as well as its resistance to extinction, are increased. Also, according to Hullian theory, the acquisition of simple responses or habits, such as conditioned eyeblink, should be facilitated by an increase in drive. On the other hand, learning or performance of complex tasks, such as discrimination learning and serial rote learning, should be affected adversely by an increase in drive due to the activation of interfering response tendencies which, under low drive, remain below the threshold of reaction evocation.

The bulk of present research has consisted of testing various hypotheses derived from these conceptions. On the experimental side, there are principally three types of procedure. One makes use of questionnaires, usually the MAS, composed of items considered symptomatic of anxiety. High and low scoring Ss on the MAS are compared on their performances on learning, motor, and perceptual tasks. Another method utilizes some form of threat or noxious stimulation, and compares the S's performance under threat and non-threat conditions. There are two main classes of threat (or stress): ego threat (e.g., failure in a competitive situation) and noxious stimulation (e.g., electric shock). The third general method combines the other two. That is, the performances of high and low scoring MAS Ss are compared under threat and nonthreat conditions. This procedure is associated with the idea that the MAS measures anxiety potential as well as chronic manifestations of anxiety, so that high scoring MAS Ss should show greater reactivity to threat than low scoring Ss.

The research prior to the current year based on comparisons between high and low MAS Ss has shown briefly this: (a) There is a positive relationship between rate of eyeblink conditioning and MAS (5 studies). The correlations between MAS and eyeblink conditioning average about .25, indicating that very little of the variance in eyeblink CR can be accounted for by differences in MAS. Spence and Taylor have attributed this correlation to the effects of anxiety as a drive. A different viewpoint is that of Franks (37; see also 25), who has argued that the correlation is due to the I-E dimension, on which the MAS has a small loading; he thereby explains these findings in terms of Eysenck's theory that cortical inhibition is the underlying cause of I-E. At present we are lacking sufficient evidence to decide whether the Taylor-Spence or the Eysenck theory is more satisfactory. (b) There is no evidence of a correlation between MAS and PGR conditioning. (c) In

differential conditioning MAS was positively related to the excitatory strength of the positive CS (5 studies). (d) MAS has been found to be related to serial verbal learning. High MAS Ss were superior on serial lists of nonsense syllables of low intralist similarity and high association value. (e) High MAS Ss require more trials and make more errors in learning verbal and stylus mazes. (f) High MAS Ss have shown greater stimulus generalization, but only when Ss were given strong shocks during their performance. (g) The evidence is very contradictory regarding the effects of stress (or threat) on performance of high and low MAS Ss, so that no conclusion is possible. (It should be pointed out that there is some contradictory evidence for every point mentioned above.) (h) The MAS has shown correlations with psychiatric ratings of about .3 to .4 on the average, approaching an upper limit of about .6 in a couple of studies. For a full account of the major research findings with the MAS, the reader is referred to the review by Taylor (88).

This year has produced many contradictory findings, especially when the MAS was used as the measure of anxiety. This measure has not fared nearly as well this year as it has in the past. For many a positive outcome reported in the literature one can find its negative counterpart. This fact points to the urgent need for greater standardization of experimental procedures in this area. Too few experiments are sufficiently comparable to permit an evaluation of their contradictory results. Under such conditions even findings from various studies that appear to be in agreement cannot be integrated with confidence.

A series of carefully executed experiments by Farber & Spence (33) seems to sum up the situation on the negative side. Using reaction time (RT) as the dependent variable and MAS scores as the independent variable, Farber & Spence failed to find that variations in anxiety level affected RT in any manner, "either as a main effect, or as a function of stress, task complexity, stimulus intensity, or generalization. The effect of experimentally induced stress was also unclear" (33, p. 17). In short, the main relationships between anxiety and performance predicted by the Spence-Taylor conception of anxiety as a drive were not borne out. Probably because of differences in experimental procedure, Castaneda (11) found a significant but complex relationship between the MAS and simple RT; the relationship was positive for a CS (sound) of weak intensity and negative for strong intensity of CS. Response amplitude, as measured by a hand dynamometer, was positively related to anxiety.

In serial rote learning of nonsense syllables high MAS Ss were little affected by threat (shock) (81), but were adversely affected by failure (ego-threat) (79). However, high and low MAS Ss did not differ in serial learning when failure threat was not introduced (79). A threat of shock that could be avoided improved the performance of low MAS Ss, but their performance was impaired by a shock that could not be avoided. High MAS Ss did not show this difference, which was interpreted as suggesting that anxious Ss do not respond adaptively to threat (81). High MAS Ss also showed less in-

cidental learning (80, 81). In learning lists of nonsense syllables some of which were made up of similar (competing) and some of dissimilar (noncompeting) syllables, the high MAS Ss did relatively better on the competing lists than did the low MAS Ss, which is opposite to the outcome predicted by the Spence-Taylor theory that more interfering response tendencies are brought into play by higher drive level (78). High and low MAS Ss showed no differences in learning paired associates (meaningful words+nonsense syllables), although those paired associates that consisted of an emotionally charged word took longer to learn, on the average, for all Ss (46). Anxiety was found to have disruptive effects on more complex intellectual tasks, viz., timed intelligence tests and abstraction (80); and schizophrenics showed more disorganization in solving emotionally charged dissected sentences and arithmetic problems than in solving neutral ones (45).

Anxiety (MAS) was not found to affect stimulus generalization as would be predicted from drive theory (33, 77), but high MAS Ss did show more stimulus generalization than low Ss when given strong electric shock; weak shock or buzzer had no effect. Parallel results were obtained when psychiatric ratings of anxiety were used instead of the MAS (77).

In the perception field, the MAS and Sarason Test Anxiety Scale were found to be positively associated with delay in recognition of tachistoscopically presented words, but there was no interaction between anxiety level and threat as produced by emotionally charged words (85). On the other hand, there was evidence of increased sensitivity (perceptual vigilance) to anxiety associated stimuli (Blacky Test pictures presented tachistoscopically) (86). Threat induced (ego-threat) anxiety enhanced perceptual constancy; it increased frequency of closure, perceptual rigidity, and speed of establishing a stable configuration. The MAS, however, was related only to the stability test (69).

It will be noted that in nearly every case in which significant relationships are found and can be interpreted in terms of drive theory, the results are produced, not by differences in MAS, but by experimentally induced threat. It appears that the kind of anxiety measured by the MAS or by clinical ratings of anxiety is activated as a drive variable only when threatening or noxious stimuli are present; and where anxiety as measured by the MAS does show an effect, it is always weak compared to the effects of threat-induced anxiety. The two kinds of anxiety seem to have little variance in common. The MAS correlates as highly as its own reliability with measures of general neuroticism; it apparently taps chronic symptoms and defenses more than the kind of anxiety that has drive properties. In fact, it can be argued that the latter kind of anxiety may always be situational and that its effects cannot be studied, or even demonstrated, unless it is aroused in relation to the phenomena under investigation. The anxiety that originally caused and perhaps sustains neurotic symptoms may not be a relevant drive in the laboratory experiment. If anxiety is an emotional reaction, it should be reflected in certain autonomic functions, and the evidence regarding the MAS

and Sarason Test Anxiety Scale on this point is completely negative; zero correlations have been found consistently between these questionnaires and such indices of autonomic reaction to threat as GSR, the Palmar Perspiration Index, and respiratory activity (8, 9, 63, 71). The use of questionnaires as criterion measures of anxiety would thus seem questionable at present. The systematic validation of such questionnaires would call for experimental work along the lines suggested by Kamin (54). He rightly maintains that a test of anxiety must predict the magnitude of the effect on a S's performance of a change from nonthreat to threat conditions; also there must be an adequate sampling of performance tasks and of threat conditions if the anxiety measure is to have any generality. Whether or not such a measure of anxiety would be correlated with clinical assessments of anxiety would remain to be seen.

In many anxiety experiments one variable that often shows the greatest statistical significance is that of sex differences (8, 11, 33, 46, 69). There is nothing in theory that would account for these sex differences and it would seem to be an interesting enough phenomenon to merit further investigation in its own right. At least it must be taken into account in the design of experiments.

The biggest gap in anxiety research is the lack of investigations of the reinforcing properties of anxiety reduction on the human level. A search of the literature of this year and of past years has not turned up a single experimental demonstration with human Ss of this theoretically most important function of anxiety. Anxiety responses have indeed been conditioned to previously neutral stimuli in human Ss, but anxiety reduction has never been demonstrated as the reinforcement for the learning of new responses.

There is much excellent research on this subject with animals. It has been extensively reviewed by Solomon & Brush (7) in what is easily the year's outstanding paper in this field. Some of the excellent experiments cited by Solomon & Brush pose serious problems for a theory of avoidance learning based on anxiety reduction. For example, they have described a doctoral thesis by Black, who used cardiac rate in dogs as a measure of anxiety and found that the peak of anxiety follows the avoidance response (the US was shock). A well-learned avoidance response (turning the head to press a lever) did not prevent the cardiac acceleration associated with anxiety. Apparently the avoidance response was not a means of escaping anxiety and thus could not have been reinforced by anxiety reduction. Yet the rate of learning the avoidance response was related to the magnitude of cardiac elevation in response to the CS. Obviously there are many unanswered questions awaiting further research.

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THE THEORY AND TECHNIQUE OF ASSESSMENT^{1,2}

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DISCOVER OR INVENT?

It may take a while for it to happen, but surely someday scientists will be able to rid themselves of the notion that, while things can be invented ideas can only be discovered. It would be far better to believe that all ideas are sheer fabrications and that it is only the palpable things that sit around waiting to be discovered. Such a view would help to advance creative thinking to its proper phase in the cycle of scientific reasoning. It would also cast empiricism in its proper role—the discovery of what things ensue from the courageous application of invented ideas.

In the meantime, we shall continue to go poking about among our experiences pretending to uncover ideas, all the while believing that such notions as "anxiety," "hostility," or "hypochondriasis" are either the revelations of nature, or of dynamically oriented psychiatrists. As for using our inventive talents since we are, as we say, scientists, supposedly the only use for them is in the devising of such practical things as tests, gadgets, or mathematical scales which appear to conform to nature's expressed ideas.

To our way of thinking, this is about where the psychology of assessment now stands. We are still inventing a few new testing devices for it is legitimate to invent *things*. We are doing a little better in the prediction of specific criteria, which is largely a matter of refinement of technique. And we have backed away from our computational activities far enough to begin to worry about such matters as "construct validity." But if, at last, we have paused to inquire about the dimensions of man, it is still only to ask where they may be found, not to turn to the bold task of erecting them.

There are, nonetheless, a few straws in the wind. Surprisingly enough (or is it surprising?) some have blown up from the direction of mental deficiency which is, at the moment, psychology's most conceptually barren field. Cantor & Cromwell make a timely case against reductionism, the belief that descriptions should be reduced to conceptual terms that are most "real" (1). Yet, more familiar voices still recite the old convictions. Hunt, who is certainly no fool, punctuates his scholarly Salmon Lectures throughout with pledges of ultimate loyalty to physiological ideas (2).

Some look upon factor analysis as a happy compromise between discovery and invention. But it is essentially a collecting procedure, though it may, through its functional arrangements and graphic displays, make it easier for the psychologist to reconceptualize his dimensions of assessment.

¹ The survey of the literature pertaining to this review was concluded in May, 1957.

² The following abbreviations will be used in this chapter: MMPI (Minnesota Multiphasic Personality Inventory) and TAT (Thematic Apperception Test).

By including both predictor and criterion measures in the same factorial matrix, Bair, Lockman & Martoccia found themselves aided in defining the factors derived from predictor measures alone (3). There is also a new non-parametric factorization procedure resembling a code-breaking device, which permits the psychologist to begin with whatever makes personal sense to him and to assess his data in that familiar frame of reference (4, 5). But those who group data by factorization, whether by function or variance, are still attempting to discover what can only be invented (6, 7), while the whole or partial correlational procedures used by others are essentially belated attempts to put the flesh of meaning upon some very, very dry bones (8).

The challenge to invent ideas often seems to come directly from those who are assessed; some persons defy being explained, at least in the traditional terms (9, 10). One study even reports that ambiguous statements are more valid than either positive or negative statements, when forced choices are used to predict scholastic achievement (11). The revolt of subjects against the black and white values of psychologists must be well under way.

What would we do if we had to start from scratch in an assessment program on Mars? That would indeed be an invitation to invent new axes of reference. Or, to take a simpler problem instead, the assessment of vocational choice among the Apache Indians. The Rosses found this simpler problem quite baffling enough (12). They resorted to giving Rorschachs! But they promised to come back the following summer with better ideas.

THINKING

There are other harbingers of a fertile spring to come. Following by only a year Johnson's systematization of the "ill-defined field" of thinking (13), Bruner, Goodnow & Austin have come up with a series of studies firmly rooted in the modern philosophy of science (14). Conceptualization is envisioned by them as only one of the stages in a cycle which includes explicit identification of objects in terms of abstractions, decision making, and consequences—the payoff matrix of a decision. Thus, "the pale cast of thought" with which this field has so long been "sicklied o'er" may soon regain "the name of action." It is only a short step from here to the study of the psychologists' own thinking and what they could do if they put their minds to it.

Thinking has, of course, long been a topic in psychology, but much of the impetus for the new trend can be traced to Goldstein and the efforts of others like him to share the private thoughts of schizophrenic patients. Already McGaughran & Moran have challenged Goldstein's conceptual level hypothesis with a carefully defined notion of conceptual area, based on two dimensions: private-public and open-closed (15). They propose the use of an "Autistic Index" in assessment and they report data supporting the utility of their point of view.

Levy & Dugan have now reported the original study in the factorial

analysis of personal constructs (16). It was this study, which employed conventional factorization procedures, that inspired the development of non-parametric methods for coming to grips with personal constructs. Other factorization studies in the field of thinking are still based on inter- rather than intraindividual variances (17, 18).

Can people change their axes of reference? This is a question that has important bearing on the thinking of psychologists as well as that of their clients. Levy finds that they can if they do not see too much interrelatedness between their axes; otherwise it is difficult (19). This may be an important way of assessing individuals. As for psychologists themselves, this finding may mean that the more correlations they publish the more rapid their thinking will become.

The assessment of the personal constructs in terms of which people do their thinking is not limited to the laboratory or to the clinic. One free-protocol study even comes out of General Motors (20).

If it had not been for schizophrenia and its puzzling trains of thought, psychologists might never have gotten around to the assessment of thinking. Or is it the other way around? At least, schizophrenics show impairment in their thinking, we believe. Binder says it is an over-all intellectual impairment, rather than the differential impairment found in normal aging (21), and Chambers says it is an impairment in judgment without loss of accuracy in learning (22). Davis & Harrington note that the presence of other persons interrupts problem solving in schizophrenics (23). This may tie in with the privacy dimension emphasized by McGaughan & Moran (15).

Molish, still concerned with the classificatory problem of conventional diagnosis, but with a new look, seeks the schizophrenic components he may use (24). Guertin does the same by factoring the activities of schizophrenic patients (25), and Otgel classifies in terms of data that fall out of the case folders when they are dusted off (26).

Stotsky has begun the important task of developing vocational tests for the placement of schizophrenic patients, an especially important task, since it now appears that the isolated mental hospital, as we now know it, is rapidly disappearing in favor of a local in-and-out, out-and-in pattern of life for schizophrenics (27).

Gorham reports an unusually thorough study on the use of the proverbs test—a thinking approach, we are impelled to say—in screening military personnel for schizoid tendencies (28). Goldstein & Carr, apparently aroused by the current suspicions about mothers, take a look at the mothers of schizophrenic patients (29). There are no differences in the kinds of responses, but the mothers of catatonics omit four and a half times as many items as the mothers of paranoids. We wish they would pursue the matter further.

There were, of course, other studies concerned with the assessment of schizophrenic manners in terms of physiological dimensions such as as subshock insulin (30)—significant only at the "Oh Shucks!" level of signifi-

cance—;³ prefrontal lobotomy (31)—same level—; and brain dysfunction (32)—highly significant at the personal conviction level.

ASSESSMENT IN TERMS OF CONVENTIONAL CONSTRUCTS

Since, as we have seen, it is hard to change an axis of reference when it has a wide range of interdependency (19), it is not surprising that psychologists are still trying to measure in terms of such sweeping constructs as anxiety and aggression. Nevertheless, Wirt & Broen, taking a close look, conclude that what is measured on the Children's Manifest Anxiety Scale is not the same as what is called anxiety in the clinic (33). Zimet & Brackbill find this to be true of other measures also (34), and Siegman gallantly concludes that the Taylor Manifest Anxiety scores must have more construct validity than criterion validity (35). Sullivan & Calvin's findings in a vocabulary study generally support Siegman's conclusion (36).

Sinick reverses the current trend in findings on the relationship between encouragement, anxiety, and test performance (37), but Zweibelson finds that anxious children do better on a second test (38). Heath shows that anxiety goes with response disorganization; but then, so also does lack of abstract conceptual ability (39). Rorschach color takes another beating as a measure of anxiety, this time in an arabic-greco-latin square design, no less (40). Intellectual performance correlates (person-to-person variance) with Taylor anxiety, but not with changes in Taylor anxiety [within-person variance (41)]. And sure enough, just as Taylor has said, intelligent people can, if they try, cover their anxiety in taking her test (42).

Dibner has developed a measure of anxiety based on cues given in interviews (43). Ambiguity makes the interview more productive of such cues. If this holds up against outside criteria, it should be of great help in improving the sensitivity both of psychodiagnostic and of psychotherapeutic interviewing.

One more last look at digit span, an old friend in the anxiety circuit: Blackburn & Benton achieve greater reliability by giving more digit span items (44). The reader should repeat ten digits backward before he comments on this one! Goodstein & Farber hypothesized that the relation between Taylor anxiety scores and digit symbol performance was non-monotonic—but it was not (45). Interestingly enough, someone has now come up with a "short form" of the Manifest Anxiety Scale (46). Miss Taylor, shake hands with Mr. Wechsler, who knows all about how it feels to have his test reduced to "short forms!" Incidentally, neither of you need worry about clammy hands; Lotsof & Downing (47) and Calvin *et al.* (48) all assure you that your hands will not betray your anxiety.

Does aggressive fantasy aroused by pictures lead to overt acts of aggression? The fate of comic books, movies, and television may hang on the outcome of this one. Fantasy of outright fighting is at least associated with

³ An expurgated version of a fiducial limit originally used by R. E. Jones in an oral communication.

playground fights, says Kagan, but other aggressive themes are not (49). Lindzey & Tejessy also have trouble assessing covert aggression from pictures (50). But within the realm of intraindividual variance, Goldstein & Rawn show the effect of induced aggression on drawings, primarily in terms of graphomotor factors rather than symbolic representations—again something more overt than covert (51).

Two scales come in for appraisal. A study using the Palo Alto Aggressive Content Scale shows normals turning hostility outward, schizophrenics inward (52), and the Iowa Hostility Inventory gets some clinical validation, especially among women subjects (53).

Impulsivity and inhibition continue to recur as a reference axis in assessment. Two factorial studies produce displays that many well be seen in terms of this dimension (54, 55). In another, cognitive inhibition time is significantly related to Rorschach M, but not to the Erlebnistype ratio (56). Two other studies corroborate the significance of M, one showing high M to be associated with ability to inhibit laughter (57) and the other showing it to be related to the control of impulsivity, in a digit symbol test (58). Perhaps it is time that psychologists take another hard look at impulsivity and inhibition.

Empathy is another conventional construct which plays an important part in the communications of psychologists. Jarrard examines it from the standpoint of an industrial psychologist, considers its applications, and concludes that a high degree of empathic ability goes with high morale (59). Lundy, approaching it from the standpoint of assimilative projection, finds that when judges concentrate on their own behavior they are inclined to see others as more and more like themselves, but if they concentrate on others' behavior, they begin to sense how others see themselves (60). May we infer from this that psychotherapists who become overly concerned with their technique or their interpretations of client remarks, end up convinced that their clients have improved because they appear to be more like their masters? Borke & Fiske, however, find no relation between the four experimental interview conditions they tried and the way therapists empathize with clients (61).

When clients say they have improved, their therapists, as likely as not, will disagree, but when they say they have not improved, their therapists concur (62). Finally, Murstein (64), using the Dymond Individual Empathy Test, criticizes the findings of Norman & Leiding on the relationship between individual and mass empathy measures (63).

Since the publication of the Adorno report psychologists have been concerned with the construct of rigidity and have been re-examining the Einstellung phenomenon. Levitt, in a review of eight years of research, concludes that the water-jar test, as a psychological test, is a very poor one (65). It produces U-shaped distributions. From the writer's vantage point, however, this is a criterion of a good test, although we have no particular interest in filling and refilling water jars. Maher failed to find the water-jar perform-

ance related to the personal use of dynamic perceptions of other people, but he did find a relationship with achievement anxiety (66). He sees support in this for the view that situational or field variables play a significant role in the production of rigid behavior. There is a study which fails to find a relationship between tree drawings and Rorschach signs of rigidity (67) and another which discusses the general application of rigidity principles to the measurement of personality disturbance (68).

The tendency toward antisocial behavior is one of society's most urgent problems. Purcell reports that, "The superego variable as measured by anticipated internal punishments was found to be of far greater significance in inhibiting antisocial behavior than fear of retaliatory punishment . . ." (69). In fact, antisocial subjects utilized themes of external punishment as an outright justification for their fantasy aggressions. In another study he uses the Qualitative Maze Test to assess antisocial behavior tendencies and concludes that impulsivity is a basic variable in assessment (70).

Dependency also comes in for assessment. Bernardin & Jessor report a construct validation study in this connection, using the Edwards Personal Preference Schedule (71). The seeking of approval and help figured in the validated construct, but conformity did not.

Sexual identification and deviation are proving difficult to assess. Most of the measures turn out to be indications of general maladjustment. Cutter reports that sex responses to Rorschach Cards 6 and 7 are associated with the estimated severity of disturbance but not with the degree of overt sexual deviation (72, 73). From their relationship with other personality measures he concludes that sex responses to these cards are simply indications of acute anxiety. Davids, Joelson & McArthur find Rorschach "signs" in overt homosexuals (74). They offer a new Rorschach "sign" and five new TAT "signs." Sexual characteristics in human figure drawings show no relationship to sexual adjustment (75). They are more self-portraits than indications of role identification (76). The theory of cultural sex symbolisms in formal designs gets strong support (77). The MMPI sexual deviation scale also proves to be more a measure of gross maladjustment (78).

Clinical psychologists expend a great deal of effort in assessing "organic deficit." When they find it, and they often do, the train of their thought processes usually stops. But the reductionistic influence in psychological thinking is still so strong that once he has arrived at organicity the psychologist feels that he has achieved some goal.

Rosvold *et al.* (79) have developed a continuous performance test based on the hypothesis that the hypersynchrony of brain-damaged patients indicates reduced attention. This is one finding that may have some functional implications.

The free-hand drawing people are taking a second look at their organicity signs. Silverstein & Robinson report that three-fourths of their subjects appeared to represent their disability in their drawings, but experienced judges

still could not use the 55 scoring signs to match up the drawings of disabled and control subjects (80). Bolin, Schneps & Thorne tested the hypothesis that the height of the lightning scar represented in the drawing of a tree was related to the age at which personal trauma was experienced—no relation (81)! However, Reznikoff & Tomblen find signs in drawings which significantly discriminate patients with chronic brain syndromes (82). Two studies find relationships between brain injury and rotation; one study using drawings (83) and one using block designs (84). On a memory-for-designs test Wahler finds that the brain-damaged patients had fewer correct reproductions but they did not differ in the types of errors they made (85).

The spiral after-effect discriminates brain damage significantly, says Gallese (86), but not quite as well as originally reported, say Page *et al.* (87). Walker has now revised the Hooper Visual Organization Test so that it is workable (88).

Intellectual deficit is viewed from both physiological and psychological points of view by Reitan (89). He finds psychological variables more highly intercorrelated; as least Wechsler-Bellevue subtest scores are more highly intercorrelated than are Halstead's ten subtest scores. Mason reports the estimates of pre-illness intelligence of 510 patients and finds no differential relationship to their maladjustment, except in certain types of schizophrenic patients (90). Still body-bound, Jost & Epstein report that taking the Rorschach produces physiological stress (91).

A little gem comes to us from across the water which will bring a nostalgic tear to many who lived through the golden age of psychogalvanic response. O'Connor & Venables report a significant negative ($-.40$) correlation between skin conductance and Binet IQ (92). Finger dexterity correlates $.48$ with IQ and $-.21$ with skin conductance.

FACTORS THAT AFFECT TEST RESULTS IN EXTRANEIOUS WAYS

When subjects are told that a Rorschach test is intended "to discover emotional disturbances," Henry & Rotter find that they give fewer responses, especially fewer aggressive responses, and more good form (93). Phares & Rotter report that when subjects are sitting in an English class they show more preference for academic rewards (94). When they are in their Gym class they claim more interest in athletic reinforcements, and when in their Woodworking class they say they want manual skill items. Bernstein finds that the presence of the examiner makes a significant difference in TAT stories (95). Matarazzo, Saslow & Guze find that subjects are more susceptible to changes in the behavior of the individual interviewer than to the differences between interviewers (96).

There are two important notes on the interactions of judges or examiners with their clients. Levy points up the value of a factorial design to control this interaction, especially when case file data are investigated (97), while Gage, Leavitt & Stone report a careful study on the use of an intermediary

key to point up the variances which may cloud the relationships between judges and those whom they judge (98).

Social desirability makes a difference in the way subjects respond to test items, especially to those in the MMPI K-Scale (99, 100). It also affects the responses of high school subjects to about the same extent as it does college subjects on the Edwards Personal Preference Schedule (101). In studies comparing the self with the ideal self it is an especially important factor to be controlled, if the ideal is to be used as a reference point (102).

The ability of subjects to "fake" responses in certain types of inventories causes psychologists some concern. In general, however, results are not invalidated by the intentional distortion of responses (103 to 106) although Fisher & Morton report that fantasy measures on the TAT predict best those measures of hospital behavior which are least subject to camouflage (107).

Wiener reports that those subjects who are prone to be distrustful (MMPI) are significantly more impaired in intellectual behavior (108). This seems to put intellectual behavior in the social realm, or at least in the realm of intercommunication, where it probably belongs.

This brings us to the problem of assessment in the context of other cultures. Jahoda concludes from experiences in the Gold Coast that tests of abstract ability are no more culture-free than ordinary tests of intelligence (109). Sperling reports his experiences with the administration of psychological tests in Europe (110), and Wickert tells of his difficulties in testing security police candidates in Vietnam (111). Two MMPI studies, an Australian (112) and a German (113), together with a Spanish Language Form of the Oral Directions Test of Intelligence (114), are reported; interesting but not startling! Woods & Toal reverse other findings by reporting that Negroes perform better than whites on tests which require perceptual speed and accuracy (115). Beier & Hanfmann describe their use of California authoritarian projective questions in interviews with former Soviet citizens (116). The answers reflect general concern with problems of stark survival.

THE MEANING AND USE OF PERSONAL JUDGMENTS

If you don't know what is going on in a person's mind, ask him; he may tell you. This practical bit of advice to clinical psychologists is the basis of a number of assessment techniques used by psychologists, including sociometry, Q-sorts, personality inventories, and rating scales.

Bjerstedt offers a critical survey of sociometric theory and the computational procedures associated with it (117, 118, 119). Gronlund reports that teachers underestimate the halo effect from situation to situation when trying to guess how their pupils will make their sociometric selections (120). Nevertheless, there seems to be a general ability factor among teachers for this kind of guesswork (121). Maher suspected that nominators might tend to choose persons of their own age, but has to report that they don't (122).

What makes one a good judge of others? Taft says that social attitudes are the most important factors (123). Good judges are achievement-oriented while poor judges are more concerned with personal qualities in relation to social situations. Members of rural and minority groups, together with those who do not have working experience, do poorly. Fisher hypothesized that "deep" interpretations would seem less plausible to psychologists and psychiatrists, but found quite the reverse to be true (124). We love our esoteria. Bendig thought the personality of judges might affect their agreement with experts in assessing clinical case histories (125). He says he was wrong. And Sorenson & Gross warn against setting too complex a task for raters; it spoils their ratings and gets them all confused (126).

In another study, Bendig reports that intellectual, interest, and educational variables are not related to the reliability with which judges rate case histories, but intelligence and education produce greater rater bias (127). He says also that reliability of such judgments is not a general trait (128).

In a well-controlled study, Johnson & Vidulich report their experimental manipulation of halo effect in ratings (129). They were able to eliminate the interaction variance between rater and ratee, but at the expense of certain side-effects. Psychiatric diagnosis, a form of rating in currently bad repute, gets a gold star from Schmidt & Fonda (130). Psychiatrists who are concerned about such matters will be happy to read what Schmidt & Fonda have to say. Shontz reports that judges agree on what makes a healthy person, but not on what makes an ideal hospital patient (131). Two other studies report special rating procedures (132, 133), and Mann's review of experimental studies of role-playing concludes with the assertion that valid and reliable role-playing tests can eventually be developed (134).

Q-sorting has stirred up some lively by-play among psychologists with highly developed statistical consciences. Conger, Sawrey & Krause point out that inter-rater differences spuriously affected the findings in Beck's "Six Schizophrenias" (135). Stephenson replies that the factor arrays were intended to be related to theoretical considerations, rather than to certain concrete factors or classes (136). Conger, Sawrey & Krause retort that Stephenson confounds raters with patients (137).

Kogan *et al.* find *Q*-sorts affected by the close relationship (r of .89) between a social desirability factor and an emotional health-sickness factor (138). Livson & Nichols recommend *Q*-sorts forced into a rectangular distribution (139), while Jones recommends free *Q*-sorts (140). This reviewer likes to see free sorts fall into a U-distribution, for reasons that cannot be elaborated here.

Lorr, Holsopple & Turk report their carefully developed behavior scale for measuring the severity of illness (141), and Gordon, Lindley & May report the development of a similar scale for measuring change in psychiatric illness (142). Patients' leisure activities are related to their diagnosis but not to their response to treatment (143). The Discomfort-Relief Quotient does

poorly when applied to individual differences rather than to changes within a series of interviews with a single patient (144). Study habits and attitudes, a somewhat different kind of rating, significantly (r 's of .48-.51) predict subsequent academic rank upon graduation from high school (145).

TESTS THAT ASSUME THE EXAMINER IS RIGHT

When the subject is asked to guess what the examiner is thinking, we call it an objective test; when the examiner tries to guess what the subject is thinking we call it a projective device. Most intelligence tests belong to the former group, which probably accounts for the fact that intelligence testing has made very little contribution to our understanding of the psychological processes of anyone except psychologists.

The Personnel and Guidance Journal carries a series of articles on the best-known factored intelligence tests. The articles are written by the test authors and evaluated by Super (146 to 154). This series should prove worth keeping, even if the journal does take up a lot of precious shelf space.

About 76 per cent of the variance of Jastak's Altitude Quotient (based on a subject's highest scores in a battery) is associated with IQ (155). The Davis-Eells Games test turns out to be correlated with reading achievement in the middle and upper socioeconomic groups, but not so much in the lowest group (156).

It may be wishful thinking, but perhaps more new approaches to the assessment of intellectual ability are appearing on the scene. Copple has developed a sentence-completion test of intelligence that correlates quite satisfactorily with academic achievement and Binet intelligence, even within a restricted age sample (157). It may provide much more access to the child's intellectual processes than the guess-what-I'm-thinking kind of tests that psychologists ordinarily give to youngsters. In quite a different direction, Pinard *et al.* report the development of their differential tests of intelligence at Montreal (158). They attempted to incorporate some of the types of items that have proved useful in concept-formation testing in recent years, although it appears that their statistical criteria are driving them back to the older type of test item. Let us hope they will not have to retreat all the way.

There is stubborn concern about subtest analysis—one for it (159), one against (160), and one abstention (161). Gerberich has a kit of objective achievement test items, together with detailed instructions, for do-it-yourself hobbyists (162). Maurendeau & Pinard have a method for locating items on an age scale (163), and the stop-watch people have come up with a number of more or less conventional reports of intercorrelations between different intelligence tests, forms of the same tests, and between tests and achievement criteria (164 to 171). It is also useful to know that *Personnel Psychology* runs two series of information exchanges edited by D. L. Grant, one on normative data of different tests, and another on validity information.

In the area of infant testing, Wittenborn's monographs on adoptive children provide some validity information (172) and an appraisal of environmental considerations (173). Simon & Bass find higher re-examination contingencies when adjustments are made for the depressing effect of non-optimal conditions which affected the child's performance in the original test (174). The Cattell Scale provides a poor predictive index at six months of age and the effect of premature birth upon scores disappears after about eighteen months (175). Kogan gets good results with a picture information test with cerebral palsied children who cannot take the Stanford-Binet (176).

Finally, there is the usual flurry of interest in "short forms" of intelligence tests. This is an annual affair (177 to 182).

TESTS THAT ASSUME THE SUBJECT IS RIGHT

While Henry's book, *The Analysis of Fantasy*, deals with the TAT, it follows the recent trend that places greater emphasis upon understanding the psychological processes that are assessed, rather than upon the technique of assessment itself (183). Three studies throw light on the use of the TAT (184, 185, 186), and one (187) shows the sensitivity of another picture response test (188) to a realistic situation.

Two reviews assess the current status of the Rorschach test, one a review by Woltman of nine books on the subject (189), and the other a symposium (190 to 194) in which the article by Zubin, Eron & Sultan sounds the keynote (190). It is pointed out that Rorschach conceived an experiment in which inkblot space was substituted for real space. As a result, global clinical evaluations are often regarded as successful, experimental approaches rarely yield positive results, and content scales tend to show more validity than do perceptual scores. The authors say we need to develop more hypotheses about perception itself. This reviewer could not agree more heartily. Let's invent.

Bieri & Blacker find generality of cognitive complexity between one's perception of people and both his Rorschach determinants and his Rorschach content (195). Consalvi & Canter also find important cognitive factors (196), as do Sinnett & Roberts (197). Three studies place the emphasis upon the stimulus factors in the inkblots (198, 199, 200) in a way that invites attention to the importance of stimulus ambiguity in various areas of assessment.

Page finds M related to daydreaming (201), Mann finds significant relationships between M and C measures on the one hand and an association task on the other (202), but Canter finds the role of C has been overemphasized (203). Cards 4 and 7 do not remind subjects of their parents, current theory notwithstanding (204). A kaleidoscope quickly produces more useful percepts in certain unproductive subjects than do the Rorschach cards (205). So do the Projective Sequences materials (206).

Supplemental norms for the affective ratio involving cards 8, 9 and 10

are reported (207), but Maradie prefers to explain productivity on these cards in terms of the goal-spurt hypothesis (208). Rorschach validity is investigated in three studies with moderate results (209, 210, 211). Leventhal finds W and Z scores somewhat affected by perceptual training (212). The individual administration of the Rorschach yields more anatomy percentage and more responses on Cards 8, 9 and 10 (213). Beck's sampling is criticized (214) and the Howard inkblots yield more M- and more pure C than do the regular cards (215).

Of special interest is a symposium, on the use of the Rorschach as a research tool in personality development studies, in which the participants offer rather guarded statements about the future of inkblots (216 to 220). Two studies report unusual validity possibilities (221, 222), another deals with the achromatic set of cards (223) and three are concerned with dimensions that generalize across tests (224, 225, 226).

The projective use of subjects' drawings continues. There is a quality scale available (227). The use of 16 crayons, rather than the usual eight, accentuates the chromatic phase of the House-Tree-Person (H-T-P) technique (228). The Family Drawing Test shows significant differences in sex, race, and economic status (229). Children and institutionalized aged differ in their drawings of the human figure (230). Scorer reliability on the H-T-P is adequate for group prediction, but of doubtful value in clinical use (231). The Goodenough Scale is satisfactory for measuring the intellectual ability of mentally deficient adults, but not that of normals or of the maladjusted mentally deficient (232).

Tolor has normative findings on types of associations to the Bender-Gestalt figures (233), and he finds diagnostic validity when the figures are used as a recall test for organicity (234). Reznikoff & Olin (235) report similar findings. Aaronson reports a slight relationship between Bender-Gestalt recall and Porteus Maze performance (236).

Rhode has now published her book, *The Sentence Completion Method* (237), and this popular technique is being explored as to its test-retest consistency—it is consistent in certain respects (238); its use in the measurement of attitudes toward superiors and subordinates—clouded by the interpersonal remoteness of the interactionaries (239); its variations—simply using the word "because" as a stem 20 times (240); and its portrayal of defensiveness—it fails (241).

The reliability of the Blacky Test, using only the multiple choice type of scoring, is reported to be low among TB patients, except for castration anxiety measurement (242). If it were possible to combine the other three types of scoring, results might be different (243, 244).

INVENTORIES

Often it seems that people develop their philosophies in the imagery of their tools. This seems to be true of psychologists, whether they use 10 cards

marked with fluid blots, 20 cards representing people, or 550 cards containing disjunctive sentences. Consider the sentence people. Never has a group of psychologists accepted so uncritically the intuitive diagnostic statements of psychiatrists and, at the same time, complained so loudly against clinical intuition. This apparent incoherence may be explained, possibly, by the fact that these people see two quite disjunctive types of sentences involved; criterion statements on the one hand, and predictor statements, on the other.

Empiricism, which is greatly admired up Minnesota way as well as in this reviewer's household, always takes its criteria for granted. It must, for that is the only way it can come to be so tough-minded about its predictors. Thus, his simple-minded credulity in one situation permits the psychologist to be sophisticatedly skeptical in another. But he needs to watch out lest the sweeping conclusions he draws from his skepticism undermine his criterion before he knows it, as appears to have occurred in MMPI research.

Hathaway (245) and Meehl (246), having well passed the point of no return on this flight of discourse, and supported by the kind of literalistic study that Wirt reports (247), ably present the Minnesota point of view, perhaps better than it has ever been presented before. Dahlstrom reviews 1955 research in clinical psychology through the Minnesota spectacles (248) and, with Welsh (249), offers 66 basic readings and 700 references on the MMPI. Kimber has an alphabetized list of the MMPI items, in case someone drops his deck (250). Four articles deal with response bias, one in comparison with another test (251), and three expressed in terms of Berg's Deviation Hypothesis (252, 253, 254). The MMPI is also checked out against: feelings induced by music—scattered correlations (255); escaping from prison—it works (256); firemen versus fire captains—it doesn't (257); hysteria and K-corrected hypochondriasis—it predicts psychiatrists (258); and the Edwards Personal Preference Schedule—maybe (259).

Gough's California Psychological Inventory, with its 18 scales and its announced emphasis upon what makes psychological sense, is now ready for mass use in the schools (260). There is a biographical inventory with 10 scales (261, 262), an activity inventory based on Kuder categories (263), and a short-cut inventory comprising conventional items (264).

There are some special criticisms of individual tests. Barry concludes that the McKinney Reporting Test needs further refinement (265); Tamkin questions the construct validity of Barron's Ego-Strength Scale (266); the usefulness of the Mooney Problem Checklist is upheld (267); and there are some additional normative data on the Edwards Personal Preference Schedule (268).

SPECIAL ASSESSMENTS

Interviewing and observation are still the basic assessment procedures, and tests are best regarded as special refinements of them. Hunt, Herrmann & Noble report that brief psychiatric interviews prove valid in two of the

three types of military situations they studied (269). Merton, Fiske & Kendall have a manual for interviewing that is directed toward determining the reactions of persons to situations (270); Wittenborn reports interviewing techniques in his study of adoptive children (271); Back looked for rigidity in factual interviewing (272); and Yonge is starting a campaign to determine general interview effectiveness (273). You can tell a captain's blood pressure by what he thinks he hears in the murmurings of indistinct speech (274). Bass has a disguised personality test based on proverbs (275), and Phillipson has a TAT-type object relations technique (276).

"If you could not be human what kind of animal would you like to be and what kind would you not like to be?" Pigem's interviewing of children is based on this type of question (277). "What could not possibly be happening in this picture?" This kind of question peps up the repressed psychic content of TAT protocol (278). Peixotto tells us the popular responses to the Despert Fables (279).

Dorken reviews the Mosaic Test literature of the last five years (280) and Levin reports sweepingly negative results with it (281).

Psychologists may do a better job when they think less of pure diagnosis and attempt more assessments of prognosis, as in recovery from tuberculosis (282, 283); in the effectiveness of psychotherapy (284); in the relationship to the therapist (285); in postsurgical invalidism (286); in rehabilitation of the blind (287, 288); in alcoholics' response to drug therapy (289); in the development of administrative work patterns (290, 291); or even in the progress of graduate students in psychology (292).

Always there is the self. But Levy's neat results lead him to believe that we have overgeneralized it (293). The generality actually lies in the discrepancy we see between what is actual and what is ideal. Perhaps this principle explains the essays written by the 18 self theorists and published in Moustakas' anthology (294). Psychologists say that men emphasize their self-concepts while women emphasize their body-images (295); that acceptance of self need not balance acceptance of others (296); that one's identification with his parent is in terms of traits rather than of sex (297); and that persons in management positions are more realistic in estimating their own proficiency while those in semiskilled and unskilled positions rate themselves in reverse (298).

In a free society, where one can gravitate toward an occupation that suits him, the job may, in part, be a valid measure of the man. In such a society, if we ever achieve it, the study of occupations will become intimately associated with the assessment of personality from all angles. Roe's book, while written for today's conditions, makes particular sense in the light of this speculation (299). So, also does the report of an extensive interdisciplinary project on the conceptual framework of occupational choice (300). Darley & Hagenah's book on vocational interests should be examined in this

light (301), as should the book by Jacques (302), and the analysis by Ghiselli (303).

There are new theories afoot which may profoundly affect assessment in the future. If G. S. Hall captured our imagination with the phrase, "Ontogeny recapitulates phylogeny," so Kragh may turn the tide of thought with the notion that each perceptive act is a momentary recapitulation of ontogenesis (304, 305). There is Leary with his emphasis upon the time-binding of impulse (306). Brower & Abt's second volume on progress in clinical psychology has been published (307). Stern, Stein & Bloom have published their book on methods of personality assessment (308), and Marzolf has produced a timely volume on assessment in the schools (309).

Kavruck sketches the history of test development in the U. S. Civil Service Commission (310), and Guertin, Frank & Rabin summarize the research since 1950 on the Wechsler-Bellevue Intelligence Scale (311). The Veterans Administration has a directory of its medical research (312). Two basic articles on assessment, one by Horst (313) and one by Cronbach (314) should keep psychologists from becoming complacent about either their statistics or their French. Finally, everyone should know that the animal folks have taken up assessment (315). At last the psychology of assessment is pure!

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PSYCHOTHERAPY^{1,2}

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A review of the topic of psychotherapy can be an exhausting and exhaustive process, for this is a subject on which the psychological literature is quite extensive. It is necessary to limit oneself to reporting only the highlights. Many articles are repetitious, and many do not say anything very new. For purposes of this review, the significant material has been culled to about one-third its original size, and will be presented according to the following outline:

- I. Theoretical contributions
 - A. Interpersonal theory of behavior
 - B. Cultural influences in psychotherapy
 - C. Schools of therapy, new and not-so-new
 - D. Group dynamics
 - E. Transference
 - F. Miscellaneous theoretical "analytic" matters
- II. Reports on research
 - A. Reviews and research programs
 - B. Studies of the therapist's or client's personality or both
 - C. Studies of prediction
 - D. Research studies of process
 - E. Studies of therapeutic outcome
- III. Applications and techniques
- IV. Summary

THEORETICAL CONTRIBUTIONS

Interpersonal theory of behavior.—Perhaps the most important clinical book to appear this year is Leary's *Interpersonal Diagnosis of Personality* (1). Although primarily a book on diagnosis, it actually is important to the therapist for its theoretical and methodological implications relevant to research in psychotherapy. The thinking of Leary and his associates at the Kaiser Foundation Hospital at Oakland, California, is known to clinical people because of a number of articles which have appeared in the past few years. This book is an extensive report on the general nature of the research which has been carried out, and an elaboration of the theoretical principles which

¹ The survey of the literature pertaining to this review was completed in May, 1957.

² In this chapter the following abbreviations are used: MMPI (Minnesota Multiphasic Personality Inventory) and TAT (Thematic Apperception Test).

have constituted the foundation structure for that research. Several collaborators in this group (2) have also discussed the scheme. To evaluate and report such a massive contribution in this small space would be difficult. Only parts can be described, and those will be somewhat restricted to therapeutically relevant ones.

The aim of the research was "to develop a multilevel model of personality and to present a series of complex techniques for measuring interpersonal expressions at these different levels of personality." The approach employed was called dynamic behaviorism, referring first to the impact the person makes in his interactions with others, and second to the interaction of psychological pressures among the different levels of personality. More than 5000 cases are reported to have been studied in the production of the material. The five levels of personality evolved and discussed are: (a) level of public communication, (b) level of conscious communication, (c) level of private communication or preconscious symbolization, (d) level of unexpressed or unconscious (not considered in the study), and (e) the level of values and the ego-ideal.

After extensive analysis of all possible behavior mechanisms, the investigators finally arrived at a group of eight, which constituted nodal points on the circumference of a circle representing all possibilities. These eight are: managerial-autocratic, responsible-hypernormal, cooperative-overconventional, docile-dependent, self-effacing-masochistic, rebellious-distrustful, aggressive-sadistic, and competitive-narcissistic. In each pair of words the first is the moderate or adaptive adjustment, and the second is the extreme or pathological adjustment. For each adjustment, the scheme provides a descriptive statement of the reflexive action involved, the type of behavior this type of reflex tends to "pull" from other persons, and the extreme or rigid form which the pathological adjustment may take. People may be plotted with trigonometric exactitude on coaction charts, with scores being based on their specific performances on various measuring scales.

The aspect of this system exciting to the therapist is the fact that Leary and his associates have demonstrated in great detail the manner in which almost any verbal protocol material can be scored in terms of these interpersonal effects. This includes individual psychotherapy interviews, group therapy interviews, play therapy interviews, TAT and MMPI scores, and an interesting Interpersonal Adjective Checklist developed at the Foundation. Leary gives extensive discussions of the means of producing such scores, along with the diagrammatic plotting of their results on cross coordinate charts. Thus it is possible to plot a patient's location on the chart before and after therapy, as well as at any point during the process, and to compare this with his peers' evaluations of him, his own TAT or MMPI scores (on this system) and, if desired, his therapist's evaluation of him and his own evaluation of his therapist, or any other person he may know. Rarely has psychology found a way of placing so many different data into the same schematic system, and the implications of this potentiality are breathtaking!

Some of the implications of the above methodology for research in psychotherapy include not only analysis of the frequently studied therapeutic gain, which in this case can be based on many different sources of evaluation and measurement, but also analysis of the variations in transference and countertransference relationships, the changes in self-evaluation, changes in ego-ideal, and changes in the patient's evaluation of the nonself. In group-therapy situations the analysis of the interaction between each member of the group and every other member of the group is possible, as well as analysis of all the other concepts mentioned above.

Of course there is no reason why these measures need be restricted to psychotherapy situations. Leary gives extensive illustrations of their use in industry, child guidance, and various diagnostic situations. Conceivably they could also be applied to school rooms, prison and correctional institutions, and even political situations. Leary gives a large number of tables of the percentages of the various diagnostic categories found in different cultural samples (seven times as many university graduate students are rebellious types as are docile or masochistic!). He gives fascinating descriptions and illustrations of the characteristics of the eight main personality types. One of his most important contributions, especially to therapists, is the principle of reciprocal relationships. This relates to the tendency to "pull" from others a certain characteristic reaction; this has often been observed in the neurotic, but has not been as widely recognized among normals. For therapists this is a highly important idea, and needs to be applied as much to themselves as to their clients. We may eventually be able to say, with some real degree of assurance, what types of therapists are best for various types of clients. This would be progress.

Leary's book would have been a more effective one if he had given many more data from his results, more information regarding the methods of analysis of data and of drawing certain conclusions from certain observations, and more information about his samples. The report lacks desired specificity in these matters.

Cultural influences in psychotherapy.—Seward's book, *Psychotherapy and Culture Conflict* (3) presents a fresh and important idea in the theoretical thinking about psychotherapy. After a short and almost too elementary review of the development of psychoanalysis, she takes up the theme that a therapist can work more effectively if he is thoroughly conversant with the cultural milieu of the socioethnic subgroup from which his patient comes. Chapters are devoted to the economically deprived, Negroes, women, immigrants, American Indians, and Jews. Despite the fact that the problems of these different groups are in many ways strikingly parallel, there is much useful information for the clinician about the nature of specific problems relevant to each subculture. One objection which will be raised will be the point that people are people, and that psychotherapy should be person-centered and not problem-centered. There is much truth to this idea. There must not be many qualitative differences in the feeling of discrimination ex-

perienced by the Negro and the Jew. But there are quantitative differences in the status rights usually accorded different minorities, and Seward is well acquainted with many of them. Awareness of these differences may help the therapist understand the depth of feeling of his client.

It might be suggested that neither an extreme individual-oriented nor an extreme environment-oriented psychotherapy is conducive to the best handling of the cases of minority groups. Seward is likely correct in saying that being conversant with the patient's cultural pressures helps in establishing rapport, in constructing interpretations, and in understanding needs. Her critics would probably be right in saying that she may overplay the importance of these differences. All persons experience some forms of discrimination, and the clinician who cannot empathize with the feelings of a considerable number of different types of patients had best give up trying to be a therapist.

Seward gives numerous case illustrations, but unfortunately she does not give a close enough picture of the actual client-therapist interaction. Her case histories are mostly just summary reports, and the real essence of the methods used to deal with the social and cultural problems is missed. Verbatim recordings of interviews would have made the book infinitely more effective, and further theoretical exposition would have helped. It is, nevertheless, a real contribution of a pioneer sort, and will probably start a flow of ideas in a new direction.

The same sort of theme as Seward's is discussed in articles by Abel (4), Bishop & Winokur (5), Sarnoff (6), Jessor (7), and Slavson (8).

Schools of therapy, new and not-so-new.—Nine years ago this reviewer predicted in the first *Annual Review of Psychology* that the schools of psychotherapy were integrating. He still believes this, but the process has been slower than expected. The schools are still with us, although they seem to have lost their former animosity. There is much more tendency to listen to each other, and to give each other's ideas a trial. Nevertheless, Corsini (9) raises the justified question of why there are so many different schools of therapy. While he believes that personality as perceived by Freud, Rogers, and Moreno may be real, phenomenological, and manifest, he sees the schools as intellectual, emotional, and actional.

An attempt to contrast differences in thinking of some schools took place in a symposium at the APA meetings in San Francisco, where Rogers (10), Mowrer (11), Kimble (12), and Miller (13) approached the analysis of the dynamics of a child guidance case under the strict chairmanship of Shoben (14). More meetings of this type may help to reduce differences.

Rogers (15, 16) has elaborated his more recent thinking about the necessary conditions for therapeutic change. These include (a) two persons being in psychological contact, (b) the client being in a state of incongruence (conflict), (c) the therapist being congruent, (d) the therapist experiencing unconditional positive regard, (e) the therapist experiencing empathic understanding and communicating this, and (f) communication being

achieved. The stumbling block for some schools will be the idea of unconditional positive regard. However, Rogers seems closer to other schools now than formerly; or possibly it is more accurate to say that the rapprochement is somewhat mutual.

Phillips (17) has written a disturbing book about what he calls a modern theory and practice of psychotherapy. It should probably be described as a protest against depth therapies, and especially psychoanalysis. It is theoretical, but not in a good sense; rather, in this reviewer's opinion, in a confused sense. The now commonly accepted therapeutic methods of depth analysis and all the associated phenomena like abreaction, transference, and resistance, are rejected under the phrase "historical-geological-hydraulic," and in their place is substituted a rather simple, didactic type, insight therapy not unlike general semantics, and distinctly hortatory in nature. Every case is analyzed into four main components: "assertion, disconfirmation, tension, and redundancy." A reasonable, and certainly more meaningful, translation of these terms might be: ego-needs, reality-principle, conflict, and repetition-compulsion.

The treatment Phillips gives would certainly seem to be symptom oriented, and actually quite superficial. It is very much a lecture therapy, and the three fairly lengthy protocol selections could be only alarming to analysts and other depth-oriented therapists. Practically every statement by the therapist is four times as long as each adjoining statement by the client, highly intellectual, hortatory, and, at times, irrelevant. The reviewer feels about almost every statement by the therapist that it misses the really significant meaning of what the client has said.

Regarding the theoretical implications of the book, it is ambiguous in that although it purports to bring psychotherapy at last into the realm of a true science, it actually is steeped in academic philosophy. It is far from being a learning-theory conception of psychotherapy. The author admits indebtedness to theorists like Melton, J. E. Anderson, Goodenough, Lewin, R. R. Sears, Spence, D. Wiener, and the philosopher H. Feigl. A true synthesis of these persons' ideas would be an accomplishment long to be remembered, especially if applied to the art of psychotherapy. Unfortunately Phillips has not achieved a synthesis, but only a garish garden bouquet. The ideographic-nomothetic controversy is denied, by an attempt "to point out the fundamental similarity between clinical problems and statistical methodological and predictive problems." A chapter is devoted to denying the necessity of using the concept of an unconscious, which is held to be cloudy and scientifically untenable. Another chapter substitutes for the unconscious the idea of conflict theory, under which "psychopathology and psychotherapy are conveniently subsumed . . ."

It is certainly doubtful whether any simple one-concept theory can explain most of these two areas of concern, not to mention being able, as Phillips proposes, to tie them into "general psychological knowledge and the new scientific concerns such as those subsumed under cybernetics (com-

munication-information theory)." A book with the theoretical pretensions of Phillips' might be a valuable contribution if it represented the synthesis of the life work of a great scientific and philosophical thinker, but it is doubtful whether Phillips' attempt to tackle these great matters will be long remembered.

Slavson (18) has praised Freud as the "father" of group therapy, and considers all group therapy as essentially analytic. Johnson (19) sees analysis as a process of negative conditioning or reciprocal inhibition. He feels that we are at present confusing the disease with the medicine, and that the philosophical implications of the technique reflect the concept of man of the late nineteenth century Germany, and deal with a compound of determinism, atheism, hedonism, fatalism, and mechanism. All these dire imprecations, however, must be considered as pointless as swearing at the Mississippi for moving southward, if one is to judge by the volume of literature profoundly influenced by analysis. Whole journals appear regularly with pseudo-theoretical articles praising, or describing, some aspect or application of analytic thinking. It is absolutely impossible to review this sort of article in the limited space available, but there is no dearth of its kind! Even Adler makes a slight comeback in an article by Papanek (20), and in the *Journal of Individual Psychology*.

Group dynamics.—Nine years ago this reviewer predicted that group therapy would become an important therapy area, and this prediction has certainly been verified. Recently articles descriptive of method in group therapy have begun to show a trend toward consideration of group dynamics. Slavson (21) believes it important to differentiate between "group dynamics" and "interpersonal interactions." Some groups have "group egos" or common goals. In group therapy there is no common group goal, but rather individual goals. Frank (22) and Durkin (23) also discuss group interaction, and Spotnitz (24) describes it in groups of schizophrenics. It will be interesting to see how much group dynamicists, group therapists, and communication theorists can get together in the next ten years. They all seem to be talking about similar processes.

Transference.—Recently there has been an upsurge of interest in transference, or sometimes just in the therapy relationship. Although there are only a few research articles, theoretical articles on this topic abound. The reviewer read twelve articles about transference but has decided not to evaluate them because in general they do not say anything very new or creative.

Miscellaneous theoretical "analytic" matters.—Several theoretical points probably merit brief mention. Ziferstein & Grotjahn (25) suggest that acting out is more common in group therapy than in individual therapy. Cath *et al.* (26) propose that therapists must give more consideration to the concept of body image, and, especially, in cases of physical handicap, allow the patient to strengthen his intellectual and rational defenses. Rubenstein & Levitt (27) feel that more attention should be given in therapy to the role of the father. They believe that the role of the mother has been overem-

phasized, especially in child therapy. Bross (28) believes that some of the reasons that patients leave group therapy are: (a) feelings of inadequacy, (b) fear of loss of control of impulses, (c) fear of the unknown and unpredictable, (d) flight from facing the sexual drives of others, and (e) flight from any situation repetitious of a transference phenomenon. Szasz (29) believes that while much has been written about the patient's experiences in therapy, not enough has been said about the analyst's experiences.

REPORTS ON RESEARCH

Reviews and program research.—Several reviews of research should be mentioned. Cartwright (30) prepared an annotated bibliography of research and theoretical articles in client-centered therapy. Besides making a comprehensive report, Cartwright has devised a new classification system for research articles. The former two classifications of process research and outcome research seem to him no longer adequate, and he proposes six major categories: (a) discrete variable analysis of therapy, (b) multivariate analysis of therapy, (c) studies of related issues, (d) methodological developments, (e) analysis of research directions, and (f) theory construction.

Snyder (31) presents a review of 21 studies produced during eight years of program research at Pennsylvania State University. The studies reported fall into four main areas: (a) prediction, (b) therapeutic movement, (c) counselor characteristics, and (d) client-counselor relationship. In several of the studies factor analysis has been employed in rather unique ways, i.e., factoring therapists into clusters, and factoring measures of outcome. Snyder reports an important development observed in several studies, that the interaction between therapist and method relates significantly to outcome, whereas neither of these relates independently to outcome. He criticizes univariable research as being unrealistic in a complex relationship like therapy. Both of these points have been observed in several other research reports, and probably indicate the beginning of a trend, as is true, also, of the use of factor analysis in therapy studies.

A report of a major research project in psychotherapy comes from the Menninger Clinic. The scope of this program makes it undoubtedly one of the most important now under way. Four collaborators, of whom Sargent and Luborsky are the psychologists, report the plan in an issue of the *Bulletin of the Menninger Clinic* (32). Most of the research is being carried out within the framework of the regular treatment activities of the Clinic. Sargent identifies four levels of clinical research, which might be called: (a) clinical observation and hypothesis finding, (b) process or covariant studies in formulating hypotheses, (c) factorial or hypothesis refining, and (d) experimental studies or verification. She identifies the Menninger studies as being primarily at the second or process level, beyond that of clinical "hunches" but beneath factorial or experimental ones. The choice of level of study at the Menninger Clinic is partly based on the present rather unsystematic state of the whole field of psychotherapy, and partly on the clinical

need to disturb the patient as little as possible in the process of measurement and therapeutic activity that must occur. Also, the activities of the Clinic must meet the requirement of applying small sample methods. Citing an article by Edwards & Cronbach as justification, Sargent defends the use of a large number of independent variables being observed and controlled, as more valuable statistically than a large number of subjects.

Robbins & Wallerstein, psychiatrists, indicate that the major hypotheses of the Menninger study include the following: (a) a rational (theoretical rather than empirical) analytic psychotherapy effects a resolution of intrapsychic conflict through recreation in the transference situation of the whole complex of the client's conflicts and defenses, to which the therapist responds in a manner different from the usual responses; (b) psychoanalytic therapies include a gamut of methods which are combined in unique clusters to provide the different forms of treatment; (c) different types of therapy are specifically and differentially helpful; (d) different predictions can be posited about the outcomes of therapy, based on such factors as variations in transference, change in symptoms, general behavioral changes, structural changes in the ego, and the acquisition of insight; and (e) it is possible to determine which attributes in the professional personality and behavior of the therapist interact with which variables in the patients, and what the outcome of these will be.

Four main groups of treatments are described: (a) psychotherapeutic counseling, which is information-giving; (b) supportive psychotherapy; (c) expressive psychotherapy, which "projects problems onto the transference screen . . . but does not analyze transference and resistance back to the genetic-dynamic roots"; and (d) psychoanalysis, which is "the production by a neutral analyst of a regressive transference neurosis and the ultimate resolution of this neurosis by techniques of interpretation alone." These treatments are used in different situations, and their choice is an important variable for study at Menninger.

Most of the research, as reported by Sargent, is based on information drawn from extensive forms prepared during the lengthy diagnostic work-up which patients receive at the Clinic, or upon careful evaluations during or after therapy. These evaluations are made not only by the therapists but more frequently by other judges.

The work at Menninger is undoubtedly important, and there will probably be significant findings. So far little more than the research plan has appeared. Many investigators will be waiting eagerly for some results to be published. It is perhaps somewhat regrettable that the research plan at Menninger is focused mostly around hypothesis formulation. It would also be desirable if the actual material of the psychotherapy itself were to be studied more, rather than the changes in the patient, or the interrelation of variables in the makeup of patient and therapist. It is a bit unfortunate that the therapy itself is apparently to be treated in such a global fashion.

To the reader interested in psychotherapy, one important production worth knowing about receives very little publicity, and is looked upon as only an informal publication. This is the series of *Counseling Center Discussion Papers* distributed by the group at the University of Chicago. These papers are hectographed and circulated privately in order to stimulate discussion. They are considered to have the tentative character of informal conversations, and are not permitted to be subject to critical appraisal or comment in publications without permission of the author. They include speeches presented at professional meetings, early drafts of papers intended for later publication, and sometimes just informal discussions which may later reach the public in quite altered form. However, they are consistently very interesting. Material of a good bit of theoretical and methodological importance appears, particularly with reference to research in psychotherapy. The *Papers* are quite general in subject matter, and in no sense are they down-the-line propaganda for client-centered methods. Typical recent ones have dealt with topics like:

Counselor Ratings of Process and Outcomes in Client-Centered Therapy
The Process and Outcomes of Play Therapy
Methodology in Evaluation
Effects of Drugs on Emotional Responses and Abnormal Behavior in Animals

The significance of the existence of these publications needs to be considered. Does it mean that there are not enough outlets for publication about matters of concern to psychotherapists? Or does it mean that we have arrived at the state of expecting our published material to be so formalized, so proven, that people with new ideas would be reluctant to try them out in standard journals until they have had some private circulation? If this is true, many significant ideas may be lost in the birth process. At any rate, this reviewer believes that the idea of discussion papers is an innovation worth copying in other universities. It may produce the salutary effect of causing some monsters to be stillborn, and it may encourage some rather feeble but bright ideas to be developed; either result would be desirable. Perhaps we are ready for a new medium of communication in our profession.

Another worthwhile but little known publication is the *Notice of Research Project* of the Bio-Sciences Information Exchange. This organization is a branch of the Smithsonian Institution which prepares and distributes to qualified persons abstracts of projected and ongoing research in clinical psychology, and other sciences, for which governmental subsidy or sponsorship has been requested. Thus, all applications for research grants which are submitted to the National Institute of Mental Health, and all sponsored research in the Veterans Administration are abstracted. The list of recipients of this publication is restricted, however, to chiefs of service in governmental

agencies, directors of subsidized research, and several other groups. It is unfortunate that it is not made generally available to all researchers interested enough to request being listed. Many would be perfectly willing to pay for a "subscription" in order to find out several years in advance of probable publication what sorts of research studies are being done. Considerable useless duplication might, in fact, be avoided in that manner.

A review of research, and an annotated bibliography on group therapy are presented by Sherwood (33) and by Corsini & Putzey (34).

Studies of the therapist's or client's personality or both.—Therapists' anxiety level, self-insight, and therapeutic competence were evaluated by Bandura (35) using self ratings, ratings by one another, and by supervisors. Overanxious therapists were rated as less competent than nonanxious ones, but no significant differences were found on ratings of self-insight. Self ratings did not relate to competence, and it was concluded that although the presence of anxiety affects the ability to conduct therapy, self-insight into this anxiety is not sufficient to counteract its effect. Arbuckle (36) used the MMPI, Kuder, and Heston Personality Inventory with 170 counselor-trainees and found that students who were preferred as counselors were more self-confident, more normal, and had interests in social service, persuasive, literary, and scientific activities. Specific desirable and undesirable personality traits of counselors were reported.

Turning to patient's personality, several investigators have been concerned with what makes some clients stay in therapy while others drop out rapidly. Rubinstein & Lorr (37), working with 128 veterans, observed that remainers are less nomadic, less impulsive, less rigid, and more self-dissatisfied, more educated, and from a higher socioeconomic level than were terminators. Frank *et al.* (38) studied 91 psychiatric outpatients, and found that remainers tended to come from higher socioeconomic classes, have more education and better occupations, to show signs of fluctuating illness with manifest anxiety, readiness to communicate, suggestibility, social integrity, and perseverance. He also believed that certain therapy methods and therapist personalities were more conducive to patients staying in therapy than were others. Imber *et al.* (39) found that among 57 neurotic patients, staying in therapy was correlated with suggestibility, especially in middle-class patients.

Lorr & Rubinstein (40) sought to confirm, on a population of 215 veterans in psychotherapy, a group of 10 personality and symptom patterns previously isolated by factor analytic methods. Seven of the factors originally hypothesized were confirmed fully, and the other three partially. They are (a) emotional responsiveness, (b) hostile, suspicious rebelliousness, (c) depressive tension, (d) sense of personal adequacy, (e) drive toward long-term goals, (f) compulsiveness, (g) latent schizophrenic tendency, (h) gastrointestinal reactions, (i) respiratory disturbances, (j) sex anxiety. The second order factors are equally interesting: (aa) anxiety and (bb) hostility. Saul

& Sheppard (41) developed a hostility scale from the content analysis of 500 dreams of 200 subjects, which was able to discriminate successfully between hypertensive and normotensive subjects.

Studies of prediction.—Three studies have been made which relate to predictability of staying in therapy or to outcome. Libo (42) devised a short picture thematic apperception test which showed better than chance capacity for predicting a client's return for further therapy. Its disadvantage is that the task requires almost half an hour and yields only what is called an attraction score—something that might possibly be obtained as easily by a short post-interview rating scale. Wirt (43) tried using judges' evaluations of MMPI profiles and mechanical sorting of scores on Barron's Ego-Strength Scale as a means of predicting improvement, and found that the mechanical sorting was much more reliable than the judgments of MMPI profiles, which were little better than chance. Knopf (44) reviewed the studies on the use of the Rorschach as a predictor of outcome of therapy or continuation in therapy, and concluded that results have been negative or highly equivocal. He attributes this to the problem of trying to predict one unknown from another!

Research studies of process.—Studies of the therapy process constitute the largest single group of research articles. An interesting procedure for process analysis is described by Murray (45). It is a method of content analysis of verbal behavior of patients and therapists consisting of 19 categories for patients' content and 10 for therapists'. Reliable unit measurements are achieved, and the method has high reliability; it is said to show validity on the basis of studies of recorded cases. A somewhat similar type of study is one by Fisher (46) who constructed rating scales for depth of interpretation and plausibility of interpretation. Four groups of judges obtained reliable ratings on these scales, which may be measuring the same underlying construct. Ratings made by naive graduate students correlated .88 with depth ratings of experienced therapists and .86 with psychiatrists' ratings, suggesting that advanced training in therapy may not be a prerequisite of judging what is "deep" in therapy. A short study by Mahl (47), based on only two cases, suggested that two measures of disturbance (ratio of disturbance to words spoken, and seconds of silence to seconds available) discriminate "something" between and within sessions for a given patient. These studies reveal that attempts are still being made to find better ways of analyzing interview material, although in some instances the new methods are not too profound.

Standal & van der Veen (48) found, in a study of 73 clients with a mean number of 31 interviews each, that there was a .58 correlation between length of case and movement toward personal integration. Matarazzo, Saslow & Guze (49) studied the stability of reaction of patients interviewed by two different doctors on the same day, and found that a patient's interaction pattern is susceptible to planned changes in a single interviewer's

behavior, but that this pattern is remarkably stable or invariant from one interviewer to another. Other factors studied were frequency of patient's action, and average duration of such actions.

Three studies concerned with insight development were those of Coons, Temerlin, and Grigg & Goodstein. Coons (50) hypothesized that a technique of therapy which stresses interpersonal interaction in the absence of insight would be superior in effecting improved adjustment to a technique which stressed insight with minimal interaction. He used three groups—an interaction, an insight, and a control group. The interaction group simply discussed everyday activities, and yet there were greater changes on the Rorschach and the Wechsler-Bellevue than for the insight group or for the control group. Sixty-six subjects were used and a blind analysis matching technique on the Rorschach to insure objectivity. This study should give support to advocates of relationship therapy, and should discourage those who support the importance of understanding and insight. This is a major issue in the field of therapy.

Temerlin (51) chose groups of subjects on the basis of ratings of flexible and productive versus rigid and unproductive behavior. He concluded that the greater the individual's tolerance for experiencing himself the more variable and spontaneous is his behavior in unstructured situations. Grigg & Goodstein (52) mailed a follow-up questionnaire to 288 terminated clients and asked them to check the techniques of their counselor, the results of their experience, their feelings while being counseled, and to indicate what feature of the counseling had helped most, what they had learned from therapy, and what evidence they had that therapy had produced changes. The investigators found that the four most successful counselors used different therapy approaches, and that it was not the techniques utilized, but much more complex factors which determined success. Clients expressed more satisfaction when counselors played more active roles. These investigators found no significant relationship between therapy length and the acceptance or approval of the therapy by the clients.

Four studies of physiological correlates of therapeutic processes appeared during the year. Dittes (53) studied extinction during psychotherapy of galvanic skin response (GSR) accompanying embarrassing statements. The study was well conceived and carried out, and showed that statements about sexual behavior were accompanied by strong GSR during the early hours of therapy, but showed a progressive decline during the course of therapy; there were some exceptions in certain situations in which strong transference was suspected. Anderson (54) studied heart rate during therapy and found that tension from interview to interview follows an inverted V pattern. An increase in tension is demonstrated when the client talks about himself, refers to present feelings, or expresses a negative or ambivalent emotional evaluation of his experiences. Decreases in tension are not necessarily associated with personality reorganization. DiMascio *et al.* (55), also

using heart rate, found that the higher the number of tension scores for the patient per interview, the higher his heart rate, and also the higher the heart rate of the therapist! On the contrary, when antagonism occurred, the patient's heart rate increased, but the therapist's slowed. Dibner (56) developed two speech scales, or measures of cues of transient or situational anxiety, and found them to be distinct behavioral measures of anxiety not necessarily correlated with GSR responses. He shows preference for the speech cue methods over the physiological ones. Several physiological studies have been appearing each year, but in distressingly small quantities. Psychologists often become so involved with analysis of conceptual behavior that at times they ignore the significance of somatic responses.

A number of evaluations of different therapy techniques have appeared. Using a large group of patients, Whitehorn & Betz (57) showed that skilled therapists were more successful with their cases whether or not they combined insulin with their psychotherapy, whereas the less skilled therapists had better results with a combination of the two. In general the skilled therapists were tactically more active and personally involved in their therapy. Tucker *et al.* (58) applied therapy in mass form to schizophrenics over closed circuit television, and used experimental and control groups to show that patients treated in this manner improved in communication, interpersonal relationships, socialization, self-care, work, and recreational activities as rated on a hospital adjustment scale. As with many evaluation studies, however, there was no control of the subjective biases of the raters.

Four evaluation researches on psychoanalysis are reported. Mintz *et al.* (59) reported that personality changes that take place during analysis can be recognized through Rorschach records, but they do not appear to follow a steady and consistent progress toward certain goals. This study considered 20 cases, and controlled the matter of requiring the judges to decide which protocols were pre- and which were posttherapy. Taylor (60) reports general agreement with Cartwright (61), and on psychoanalytic cases he finds that length of case correlates with success, and that there is a general "failure zone" between the 12th and 21st interviews. Bellak & Smith (62) had two analysts predict patients' behavior for several months, and also had two analysts judge it independently with reference to the same set of variables. (This is similar to one phase of the Menninger study reported previously.) Results showed a high degree of agreement between judges and predictors. This may, of course, only mean that all "tell the same story" about what takes place in analysis. Watson & Kanter (63) found that psychological tests before and after analysis agreed with judgments of therapists, patients, and observers that improvement had taken place.

Five studies considered process in group therapy. The major concern of four (64 to 67) was an analysis of the interaction in the group, and the studies were small ones. One study by Smith & Glad (68) was somewhat different. Two different types of therapist responses were considered.

Smith & Glad found that analysis of the subject's social role or activity seemed to heighten self-realization, while response to the subject's feeling seemed to arrest behavioral change by maintaining the patient's inhibitions. If this study is later confirmed it might upset some cherished notions about the principal function of psychotherapy being clarification of feeling, and may help to explicate the role of insight-producing techniques.

Studies of therapeutic outcome.—Two studies of outcome are especially significant because they are based on factor analyses. In the first, Frisch & Cranston (69), analyzing Q-sorts on one mother of a child in therapy, intercorrelated fourteen sorts and extracted three factors: (a) social acceptance, (b) struggle toward personal acceptance, and (c) hostility. The patient moved from social acceptance to self-acceptance. The therapist, supervisor, and tests all agreed on this. Cartwright & Roth (70) carried out a factor analysis of 31 cases, analyzing 10 measures of outcome. The study yielded three significant factors: (a) client satisfaction, (b) therapist view of success, and (c) client response to others. Naturally there will be some question of the validity of a factor analysis on one patient; but the approach, and its utilization in the present study, is significant. There have been only a few factor analyses of outcome in previous years.

A number of separate studies of outcome emphasize one or another aspect. In one paper, Cartwright (71) criticizes a statement in a study by Barron & Leary that therapy patients did not improve significantly more than did the waiting-list controls. He points out that there were actually significant differences in the populations from which the groups were drawn. The samples differed as to dispersion of changes. Some therapy patients deteriorated more than did the controls, while some patients improved significantly more than the controls. Saslow & Peters (72) reported that 37 per cent of a group of untreated patients were significantly improved without any therapy. Rosenbaum *et al.* (73) reported, after lengthy statistical analysis of data, that 70 per cent of their patients were improved after therapy. Improvement was found mainly in marital and work adjustment, with less in sexual adjustment and interpersonal relations. R. Cartwright (74) finds evidence of improvement in the form of greater self-consistency in successful cases than in the unsuccessful ones. Pascal & Zax (75) believed that change in deviant behavior was the important variable, and found improvement on all but two of 30 patients. Robertson (76) found that clients and therapists do not agree much when clients claim they have improved, but they do agree more when clients claim no improvement. Studies by Lesse (77) and Cowden *et al.* (78) claim markedly better success with psychotherapy when used in conjunction with chlorpromazine, especially in anxiety cases. Mundy (79) claims success in therapy with mental defectives, Fortin & Abse (80) with ulcer patients, and Ellis (81) with homosexuals. Most of these outcome studies leave a great deal to be desired. Many times the criteria of success are the most subjective sorts of judgments—sometimes by the therapists

themselves! Occasionally the criteria are more objective, and the factor analytic studies show much promise. But we may say that in general the more rigorous the criteria the less encouraging are the reported results.

Turning to outcome studies of group therapy, we find four studies which claim favorable results, using different criteria. Kotkov (82) uses patients' written statements, Tucker (83) measures reduction in soiling behavior by regressed schizophrenics, Peyman (84) uses Rorschach, Bender, and Wechsler-Bellevue, and Wilcox & Guthrie (85) use evaluations of feeble-minded girls by their matrons, who again might easily have been subjectively biased.

APPLICATIONS AND TECHNIQUES

The literature on applications and techniques is voluminous. The reviewer read more than 50 articles containing suggestions of the "how-to-handle" sort. It is apparent, however, that we have reached the stage when this sort of idea is fairly well discussed in books, and one seldom needs to turn to the primary literature for it. Reviewing all the articles of this type would be like reviewing the do-it-yourself columns in a Sunday magazine section of the newspaper.

However, there have been four new books this year which probably should be classified as technique manuals, and which deserved discussion. One two-volume book (eventually to be three) which will probably prove to be rather useful to practicing therapists is Symonds' *Dynamics of Psychotherapy* (86). It is unusual for a two-volume book to be so different in its two parts. Part I is subtitled *Principles*, and Part II is *Process*. The first is a sort of briefer form of some of Symonds' earlier works, particularly the *Dynamics of Human Adjustment*. It is a mental hygiene text, with a slant toward psychotherapeutic problems. It would not be used by therapists or student-therapists, because they would have covered the material years before in more definitive books, hopefully ones based much more on research and less on common-sense interpretations, than is Symonds' Part I. The writing is so prosaic, and most of the ideas are so elementary, as to suggest that the book should be used as a high-school text in "Effective Living." It is certainly superficial for the serious therapist. Graduate students would find it pedestrian.

By contrast, Part II would make a rather acceptable introductory text in a beginning course in psychotherapy if used with several other books. It is a sort of manual or dictionary of psychotherapy. Its focus is not so much on method as on description of process. Although seriously lacking in illustrative materials, it mentions and defines almost every therapeutic idea. It is a book which lists pros and cons on many issues, and in this sense it is theoretical. Its organization is one of its principal merits; if one wished he could comprehend it rapidly by simply reading all the headings. It is possible that the material between the many headings is a sort of fill-in; one has the impression that the author made a very detailed outline and then de-

cided to tell what he knew about each topic. The telling is in a sort of primer style. There is a tendency to present material in a dialectical manner, so that many times the reader would feel, probably justifiably, that psychotherapists are never sure of anything because there are always two sides to every question, and little proof that either side is correct. Symonds' theoretical bias is primarily a simplified psychoanalysis. Since analysis has often been accused of answering every alternative either with the argument of mechanism or reaction formation, Symonds is probably not solely to blame for the effect which the book gives of talking out of both sides of his mouth.

For those who would consider Wolberg's book too long or too expensive to use as one of the elementary texts in a therapy course, Symonds' new book (Part II) will be the answer. Its simplicity, excellent organization, comprehensiveness, large annotated bibliography, and good indexing make it a definite contribution to this field.

A controversial book will be Sechehaye's *New Psychotherapy in Schizophrenia* (87, 88). A translation of a series of lectures given at the Bürgholtsli Clinic in Switzerland, it will have some strong friends, and many violently bitter critics. Some critics will say it is not new, for Rosen said many of these things a few years ago, and many critics will say it is not psychotherapy because it is based so much on magical thinking. Extremes of theoretical approaches to psychotherapy often cluster at one side around the intellectual insight end of the continuum, or at the other end around the intuitive and emotionally produced relationship that has much that is regressive about it. There is no question but that Sechehaye belongs at the intuitional-emotional end. She admittedly deals in intuition, phenomenology, and existentialism. Most of her insights are of the sort which may be illustrated by the interpretive equivalence of apples and maternal breasts; a seriously regressed schizophrenic (our protagonist in this book) is completely reversed in her schizophrenic trend after the analyst feeds her pieces of apple (but not whole ones), talks to her in baby talk about "Mamma giving little Renee the good milk from Mamma's apples," and performs a few other similar ritualistic, regressive, and asinine acts.

But this is the sort of book that no well-read student of psychotherapy would dare to be ignorant of, if he hopes to retain any status among the lunatic fringe of his profession. It will be considered *avant garde* in many circles, and cynics like this reviewer will be labeled unperceptive or unimaginative by the intelligentsia in psychotherapeutic circles. There are admittedly some real insights in the book, as in the discussion of the intense fear and mistrust of others of which the schizophrenic is capable. And Sechehaye is certainly able to give ample testimony to the importance of early conditioning factors in the parent-child relationships, even though she occasionally overemphasizes the significance of the single, and sometimes prenatal, event.

A book that is rather unique in its application of therapeutic principles is Devereux's *Therapeutic Education* (89). It attempts to show a method whereby teachers, especially those with atypical students, can act in a manner considerably more like therapists than is usually true. Devereux is a very broadly trained anthropologist who was for a time director of research at the Devereux Schools. He is well versed in psychoanalytic concepts, and makes a strong case for their introduction to classrooms of deviate children, or for that matter to all classrooms. The book includes much illustrative material of situations where wiser than average teachers handled difficult problems in intelligent, understanding, and therapeutic ways. Many of the situations are focused on problems of sexual behavior and disciplinary infractions. This, however, seems justifiable in a practice dealing with the education of disturbed adolescents, many of whom are partially delinquent in their behavior.

Devereux has a very creative chapter discussing the difference between discipline and punishment that ought to be read by most teachers and parents; it is a lucid presentation of the difference between a positive and a negative, sterile approach to the problem of redirecting undesirable behavior. The book has numerous suggestions of unique "methods" of handling specific problems. These would likely be most employable in situations approximating group therapy, and in the classroom, but there are useful contributions to the handling of interpersonal relationships in individual therapy. Again, many of these might be most applicable to the play situation, but skillful therapists would often see their relevance to work with adolescents and even with college students. The book is somewhat discursive, and the reader must search for the nuggets of ideas in masses of not as useful material. There is a general theoretical theme, but it is simply put in phrases like appreciation of the individual, encouraging the positive side of the personality, translating the unacceptable behavior into dynamic needs, and teaching social usefulness. Despite this "do-gooder" attitude, the book has a great deal of value.

Likely to be found disappointing is *Counseling and Psychotherapy with the Mentally Retarded*, by Stacey & De Martino (90). It is a collection of reprints, 49 in all, which seem to repeat *ad infinitum* the theme that the mentally retarded can be helped therapeutically, and that society in general and the mental health professions in particular ought to give much more attention to this tragically neglected segment of the population. Most of the articles are short, and only nine or ten attempt to report on any research. Most of that research is "nose-counting," but there are also some evaluations of outcome based on such general estimates as "good," "fair," or "poor." A few of the articles are more outstanding than others; chapters by Sarason, Thorne, and Axline are noteworthy in this regard. The book would be useful to professional persons working exclusively with the mentally retarded. Other psychotherapists would probably consider it too specialized, and not stimulating enough for their interests.

SUMMARY

There are a number of trends which have been developing during the past decade, and are particularly noticeable in the year reviewed. Of the research studies reported, most are accomplished by centers able to conduct program research, such as the larger universities, or the Menninger and Frazer Foundations. An occasional interesting "single" article appears, but many of the discrete studies reveal weaknesses in research design. In the psychiatric and psychoanalytic journals one continues to find very few research articles, but research in general seems to be on the increase. Despite defects still to be found in design, our methods are becoming more rigorous, and this area which was once considered unavailable to scientific investigation is now yielding an encouraging number of facts.

The major theoretical developments seem to be coming from the psychoanalysts, the learning-theory oriented people, and the client-centered therapists. There seems to be increased motivation to develop and test theory. While not all of these efforts have been of uniformly good quality, several outstanding contributions have been made.

Over the decade a number of new journals have appeared which handle therapy articles, at least in part, and there are now developing several less formal sources of distribution of information. *Psychological Abstracts* has developed new sections dealing with this topic, and there is a trend toward publication of numerous technique books about psychotherapy. Also of interest is the establishing of the American Academy of Psychotherapists. In general it appears that psychotherapy is beginning to assume the status of an accepted branch of psychological endeavor.

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COUNSELING^{1,2}

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One of the reasons for some confusion in the things psychologists write and say about counseling is that it is possible to consider it either as a *profession* or as a *process*. As a field of professional specialization within psychology, it parallels clinical and industrial psychology and has its own history, techniques, and system of values. As a process, however, it is important in many of the other fields of specialization in psychology, and in social work, education, pastoral care, and a wide variety of other human situations. Counseling, viewed as a process, is a term that parallels "psychotherapy" rather than clinical psychology. In this review I have chosen to emphasize this process aspect. In accordance with this decision I have attempted to cover the ideas that are being discussed among social workers and pastoral counselors in addition to those encountered in the psychology and education journals with which most counseling psychologists are more familiar.

DEFINITION OF THE FIELD

A number of writers have presented ideas about the nature of the counseling process and the ways in which it differs from psychotherapy. Perry (48) thinks in terms of a continuum running from counseling problems centering around role conflicts to therapy problems centering around intrapersonal emotional difficulties. Aptekar (2), from his social work vantage point, distinguishes between casework, which always involves a concrete service of some sort; counseling, which focuses on a specific type of problem; and psychotherapy, which focuses on personality change. Bordin (10) sees counseling as a facilitation of the development of the individual at all stages of his life. He thinks counseling theory is concerned primarily with the growth of a sense of identity through successive identifications the person makes with others, the self-image and ego-ideal, and the constructive activity of the ego.

In social work we can pick up this same sort of distinction between therapy and counseling in the case presentation by Berg included in the Gomburg & Levinson (27) volume on family counseling. Counseling is presented here as an "ego-centered" process in contrast with psychoanalysis. Arsenian (4) presents counseling to YMCA workers as an aid in the development of the individual, and Brodbeck (13) discusses what the YMCA should try to

¹ Most of the material covered in this review was published between April, 1956 and April, 1957. In some instances, I have included earlier publications in order to trace the development of an idea.

² The following abbreviations are used in this chapter: MMPI (Minnesota Multiphasic Personality Inventory) and TAT (Thematic Apperception Test).

accomplish along these lines. A similar distinction between two somewhat different types of helping process is made by the Viennese psychiatrist, Frankl (24), although he uses the word *logotherapy* to characterize the emphasis on work, values, and personal identity that we commonly call counseling.

An official pronouncement on the subject during the year was the report of the Committee on Definition of the Division of Counseling Psychology of the American Psychological Association (84). After reviewing the historical development of this specialty, the committee lists as distinctive emphases concerns with personal growth and development, with the harmonious relationship of the individual to his environment, and with activities designed to influence society to recognize individual differences and encourage the development of individuals. The job of the counseling psychologist involves a wide range of clients, and requires considerable collaboration with other people in his agency or community.

Super's new book, *The Psychology of Careers* (70), presents a clear definition of vocational guidance as a process "of helping a person to develop and accept an integrated and adequate picture of himself and of his role in the world of work, to test this concept against reality, and to convert it into a reality, with satisfaction to himself and benefit to society."

Synthesizing these ideas from various sources we can say that counseling is a process by means of which each person can be helped to develop and to understand his own characteristic life pattern, his own identity. Through counseling a client may arrive at answers to the questions: What sort of person am I? Where do I belong? What shall I do with my life? Counseling aims at acceptance of assets and limitations rather than at fundamental personality change. Its function is to strengthen the ego rather than to reduce pressures from the id.

To carry out such a program, a counselor needs knowledge of the developmental process throughout the whole life span. Pressey & Kuhlen (50) have brought a vast amount of this essential material about the successive life stages into one volume. Each chapter takes up one area of human concern, such as work, education, or motivation, and summarizes what is known about it.

The Sanford (58) report of the research on personality development during the college years, a large scale study in progress at Vassar College, is another milestone along this path. It seems clear that fundamental reorganizations of personality do occur during the years from 18 to 22 and that they are related to the identity-achieving process that has been discussed. Later reports from this project will be eagerly awaited.

Another book that promises to be of special interest to counselors who wish to study the identity-producing process is the report by Foote & Cottrell (22). They propose that the proper focus for research on the family is the study of the factors making for a clear sense of identity and for competence in interpersonal relations. This "competence," as they define it, is a

quite different sort of concept from "adjustment" which has occupied such a prominent place in our thinking about personality heretofore.

THE COUNSELING PROCESS

Interest continues to be shown in the nature of the counseling process itself. In two thoughtful articles, Rogers (53, 54) sets forth the ideas that have been generated by work from the client-centered point of view. The importance of the counseling relationship and of a basic growth tendency in human beings is stressed. The fundamental questions are: "Who am I, really?" "How can I become myself?" The characteristics that emerge during successful counseling are openness to experience, trust in one's own organism, the acceptance of responsibility, and the willingness to be a process instead of a static entity of some sort.

Walker (76) criticizes the ideas Rogers has been expressing, tracing to Rousseau the hypothesis that man is basically good and contrasting it with the Freudian attitude. Commenting on the paper, Snyder (62) suggests that the philosophy the therapist holds may not have much influence on the therapeutic outcome anyway.

The best evidence that counseling does in fact affect the client's sense of personal identity comes from a report by Cartwright (16). She shows that *Q*-sorts before and after successful counseling indicate a shift of items into more extreme categories. Apparently clients who have benefited by counseling are willing to say with more certainty some important things about themselves.

Another client-centered research study by Anderson (1) analyzed the physiological trends (heart rate) in a series of ten sessions with one client. His hypothesis that tension would be highest in the middle of the interview series and subside later was substantiated by the results and, in general, the expected relationships with the content under discussion were found. The case was successful in terms of tension reduction, but a TAT analysis indicated that there was no basic personality change. Anderson calls attention to the fact that there are two different possible outcomes of counseling to be differentiated, defensive realignment and personality reorganization. (If we adopt the distinction set up in the first section of this review, we would say that counseling does not aim at personality reorganization; therapy does.)

Snyder's (63) account of the extensive psychotherapy research program at Pennsylvania State University is important for what it tells us about the changes in the thinking of the researchers as their work progressed. In general, work oriented toward the prediction of success or therapeutic movement was disappointing in its yield. They have shifted to a consideration of the personal characteristics of counselors and the nature of the client-counselor relationship. Differences between "reflective" and "leading" methods are not significant, but the interaction between counselor and

method is. Weitz (79) also emphasizes the counselor's personality, listing security, sensitivity, and objectivity as essential qualities.

Several investigators have tried in various ways to obtain evidence as to what aspects of personality make for successful counseling. Arbuckle (3) used a sociometric technique in a class of counselor trainees who had come to know one another well. There was considerable agreement in the choices of certain students as persons to whom one would be most or least likely to go for counseling. Preferred counselors were lower (more normal) on all the MMPI scales and higher on Kuder interest scales for Social, Persuasive, Literary, and Scientific. They also scored higher on the Heston Scale for Confidence. Grigg & Goodstein (29) made a follow-up questionnaire study of students who had come to the University of Iowa counseling service. There was no clear relationship between counselor techniques and successful outcomes, but reported satisfaction with the experience tended to accompany answers indicating comfort during counseling and active participation rather than passive listening on the part of the counselor. Since the questionnaire return was only 57 per cent, and reported satisfaction is not a very adequate criterion of successful outcome, these results cannot be generalized too far.

In another study in which the subjects were counselors in training, Poole (49) had the trainees make judgments before and after each interview as to what the client's problems were and what the counseling goals should be. Afterwards readers went over the typescripts and made similar judgments. The two sets of judges agreed fairly well on the problems but not on the goals. Since there was no relationship between counselor judgment of goals and client achievement of them, it seems doubtful whether any useful purpose is served by having counselors try to make such judgments.

A symposium (83) in which a single case is discussed from several theoretical viewpoints reminds us that there are various ways of conceptualizing personality problems and the counseling process. No conclusion can be drawn about the superiority of one of these viewpoints over the others. The research mentioned above, where the interaction between counselor and technique has been found to be the important variable, would suggest the advisability of analyzing what is going on in an interview in terms of whatever theory one finds most congenial, and proceeding accordingly, rather than forcing one's observation into any one theoretical mold.

COUNSELING AND VOCATIONAL CHOICE

Counseling psychologists have always been concerned to a greater or lesser extent with the task of facilitating wise vocational choices. This central task fits in well with our definition of counseling as an identity-developing process, because of the central importance of one's work to his self-concept. Increasingly of late the attention of many research workers has been focused on this choice process.

The general theory of vocational development that has been formulated

by Super (70, 71, 72) provides a framework and a stimulus for research in this area. A career choice is not made all at once. There are several stages, each with its own developmental tasks and array of choices to be made. In their long-range study of a group of boys from their early teens to their middle thirties, Super and his co-workers in the Career Pattern Study hope to be able to clarify these developmental stages and trace the relationships between successive choices. A research program with somewhat similar objectives is being carried on at Harvard under Tiedeman's direction (85).

A satisfactory theory of occupational choice would necessarily be concerned with ideas from social sciences other than psychology, particularly economics. A report of an interdisciplinary summer conference on the problem (9) formulates the framework for such a theory. The actual choice a person makes is seen as a compromise between what he would most like to do and what the economic and social realities of the labor market permit. Super (72) prefers the term "synthesis" to "compromise" and is interested in the contributions of learning and role-playing to this synthesizing process. Ross & Ross (56) give an account of the way in which an analysis of vocational choice possibilities for an Indian group led to a need for a comprehensive analysis of their society.

The most systematic attempt to apply these ideas to a whole occupational area was the Scientific Careers Project undertaken at the invitation of the National Science Foundation. Its object was to bring together what is known from all sources about the choice of science as a career, and try to chart a course for future research. Bachrach (6) has presented a preliminary report on the project and a monograph by Super & Bachrach (73) is in press.

Two other studies are oriented toward a developmental theory of occupational choice. Nelson (46) assessed the vocational maturity of each of 88 clients of a university guidance clinic. Maturity was defined as a stated interest in from one to four occupations, one of them in harmony with the person's aptitudes. The mature clients expressed significantly more satisfaction with the counseling than did the immature. Sinnett (60) asked counselors to rank the vocational areas represented by Strong groups and Kuder scores according to the temporal order in which realistic perceptions of them occur in the experience of the average person. The counselors also ranked them for complexity. The agreement shown by 500 college men between claimed and measured interests in each of these areas was correlated with these ranks. There was a marked tendency for the two sets of ranks to agree for the Strong areas though not for the Kuder. Disagreement between claimed and measured interests was greatest for those areas that are most complex, where realistic perceptions are late to develop. Such findings would suggest that counselors should take client maturity into consideration more than they ordinarily do in the course of their work.

The relationship of personality characteristics to occupational choice continues to be a topic of considerable research interest. Small *et al.* (61)

made a detailed analysis of students in four curricula in a two-year college. They rated subjects for 27 personality needs, adapted from Murray's list, using the TAT and a Job Concept Interview as the principal sources of information. There were need differences between these groups, too complex in nature to permit a brief summary. One generalization emerged, however, that may well lead to more research: that choice of an occupation may depend on differentiated needs, whereas success both in school and at work may depend more upon general ego-strength.

One research contribution to personality theory centering around self-concepts is the study by Steiner (64), in which students made self-ratings under different instructional sets so that certainty-uncertainty and optimism-pessimism could be assessed. Parallel sets of level of aspiration measures were obtained. The analyses of the relationships indicated that uncertainty about the self tends to accompany high goal-setting and an expectation that one's performance will be variable. Pessimistic self-appraisals tend to go with low goal-setting and intrapunitiveness about one's mistakes. The study suggests interesting counseling implications.

Hyman (33) analyzed the relationship of social status and vocational interests. Differences between social classes were significant only when he broke the groups down into subgroups differing in intellectual level. It would appear that it is the interaction between the two variables that affects interests.

Miller (43) studied the relationship between choice and values. In this group of students at a teachers' college, the most interesting finding was that the "no choice" group differed from both the "tentative choice" and "definite choice" groups in its tendency to emphasize security and prestige rather than social rewards or career satisfactions in their responses to a forced-choice scale.

A novel approach to the vocational choice problem is Ziller's (82) work with a measure of what he calls "utility for risk." This consists of a true-false test with ambiguous answers, scored for guessing. The hypothesis Ziller was working on grows out of decision theory with its differentiation of the three aspects of a choice situation: the prize, the price, and the possibility for success. The hypotheses that a group of students who had chosen sales occupations would be highest on this "utility for risk" measure, and that an undecided group would be lowest, were supported by the data.

There have been three excellent books in this area during the year. Thomas (74) gives a thorough and readable account of all aspects of the occupational structure as they affect and are affected by education. Roe (52) brings together a large amount of information about the psychological meaning of work in general, and different kinds of occupations in particular. Super (70) examines the whole field of vocational development, synthesizing material from vocational psychology with that from developmental "life-stages" psychology and from occupational sociology around the unifying central idea that a career is the implementation of a self-concept.

COUNSELING IN SOCIAL AGENCIES

Two general presentations, one by Aptekar (2), the other by Faatz (20), appearing before the period covered by this review, will be useful to counseling psychologists who wish to incorporate the thinking of social workers into their own thinking about the counseling process. These treatments have much in common with the writings of Rogers and his co-workers. They place more emphasis, however, on the concept of the *will*, which psychologists have tended to avoid, and they consider both separation and identification as aspects of the development of the self. In a more recent book by Gordon (28), the chapters on counseling would seem to indicate that social workers, more than psychologists, tend to emphasize the total structure of the client's situation (including the helping situation itself), make more of an attempt to limit the consideration of both client and counselor to certain problems, and pay more attention to the relationship between persons (such as husband and wife, mother and child) even at the risk of blurring the distinction between client and nonclient.

This focus on the interaction between individuals is most prominent in family counseling. Both Foster (23) and Gomberg (26) have stressed the importance of approaching marital problems in this way. Gomberg recommends strengthening the adaptive functions of husband's and wife's egos rather than treating two persons' separate neuroses. Beatman (7) presents a case study of the complex relationship between a mother, father, and eldest son, and explains what the plan for treatment was in this instance. One procedure which is being generally recommended by social workers in cases where a relationship rather than an individual maladjustment constitutes the problem is to have the same worker counsel all the persons involved rather than to assign each one to his own therapist.

One trend that may increasingly serve to bring social work and psychology together is the growth of interest in research in social agencies. Ripple (51) compared clients who continued in treatment for five or more interviews at family service agencies with those who discontinued treatment. Cases were rated by judges on motivation, capacity, general environmental variables, and the role played by other people. Clients who were concerned with some external problem were more likely to carry through to completion than those whose difficulties were largely internal and psychological. As might be expected, both constructive motivation and modifiability of the external environment were related to continuation, for the group of "external problem" clients. Coleman *et al.* (18, 19) made a detailed comparison of the clientele and activities of a psychiatric clinic and a family service agency in the same community. The social class positions of clients served by the two types of agency differed somewhat, but a more striking finding was that over half of the cases from the two lower class categories were closed at intake by both agencies. There was a difference in the way clients' problems were conceptualized in agency records, the psychiatric clinic diagnosing more individual personality problems, the family agency more inter-

personal relationships. Whether this difference was in the condition of the clients or in the habits of the staffs could not of course be determined from data based on case records. While many comparisons do show differences between the agencies, the similarities are perhaps even more apparent. This finding would seem to warrant more interchange of information and ideas than has been customary in the past.

COUNSELING IN THE PUBLIC SCHOOLS

One of the major contributions that counselors working in the high schools have made to the whole counseling movement is to remind everyone from time to time that counseling is needed by all, and not just by the inadequate or maladjusted. This is one of the principal ways in which the counseling function can be differentiated from psychotherapy. Counseling in the high schools is only one of many related services falling under the heading of guidance or student personnel work, and counselors are aware of their relationship to the over-all program of the school. This point of view does not, of course, characterize all guidance workers, and those who state it do not always maintain it consistently.

During the period of this review four new texts in the guidance field were published. Of the four, that of Marzolf (40) is the only one that does not to some degree adopt this orientation toward serving all students. In its emphasis on the diagnosis and treatment of maladjustment the Marzolf book is more similar to clinical psychology texts than to other books on student personnel work. A large number of psychological concepts and tools are discussed somewhat too briefly to enable readers unfamiliar with them to understand them.

Traxler's revision of a widely used text (75) places major emphasis on the tools of guidance, such as cumulative records, tests of all sorts, and reports and forms. Counseling involves the use of this systematically collected information to promote the development of all students.

In Warters' second edition (78), the different functions that make up student personnel work are considered, one by one, after an extensive introduction in which the historical, psychological, and sociological foundations of the movement are explained. Counseling, however, is presented as a problem oriented activity, the aim of which is to promote better adjustment in some individuals.

The broadest of these new guidance texts is the one by McDaniel (41). Counseling occupies a prominent place in his analysis of the total activity of student personnel workers, and the basic philosophy is clearly centered around service to all students rather than treatment for the deviant or troublesome.

COUNSELING IN THE CHURCHES

Pastoral counseling is receiving an increasing amount of emphasis. There is as yet no clear consensus among workers in this area as to where counseling

stops and therapy begins. The term "counseling" is used more often than "therapy," but no distinction is made between them except on the basis of the seriousness of the personality difficulty. Many of the papers are simply restatements of ideas that have become familiar in psychology and psychiatry. Auld (5), for example, outlines the help the pastor can give to a client under the headings of: (a) support; (b) information or advice; (c) permission for freer, more expressive behavior; (d) representing the demands of reality; and (e) clarification of problem. Rogers (53, 54) presents the basic ideas underlying client-centered therapy. Merrill (42) emphasizes the importance of focusing attention on the unique individual seeking help. None of these thoughts is new to psychologists.

Some of the ideas being discussed, however, do throw new light on the total counseling task. They are generated by the two special demands the religious worker must meet: (a) he cannot reject cases, but must try to offer some sort of help to every seeker; and (b) he is the custodian of society's values.

Thus some leaders in the field of pastoral counseling are breaking away from the idea that diagnosis is a necessary first step in the helping process, an idea which is very prominent though not universal in psychiatry and clinical psychology. Beatty (8) discusses the minister's responsibility for the healthy and well. It is not just the sick or maladjusted who need to consider and organize their deeper motives and values. Madden (39) explains how counseling attitudes and methods can be used to help a person set his religious ideas in order. Hiltner (32) in considering religious development presents an unusually penetrating account of the meaning of "development" in the general sense. A term which has been used in its present sense only since the 18th century, development signifies, according to Hiltner, "that process by which natural organic sequences and personal decisions are related (mutually or in tension) in the emergence or production of human beings."

The use of existentialist concepts in dealing with values and choices constitutes another "new slant" that counseling psychologists may get by following the work in this special area. Frankl's book (24), emphasizing the importance of logotherapy, has already been mentioned. The emphasis is on the person's active responsibility for his own life—working, experiencing, enduring. Another influential theorist is Martin Buber (14, 15) who stresses responsiveness as well as responsibility, and thus brings human relationships into the existentialist framework. Farber (21) discusses Buber's concept of "meeting" as an alternative to the psychoanalytic concept of "transference," defining "meeting" as "an undistorted encounter where each communicates with the other yet maintains his own identity."

COUNSELING IN NEUROPSYCHIATRIC HOSPITALS

The decision of the Veterans Administration to replace vocational counselors by fully trained counseling psychologists [Wolford (81)] is beginning

to produce a distinctive body of work. Perhaps its greatest significance for counseling theory is that it is producing evidence that complete mental health, a "cure" in the medical sense, is not a prerequisite for progress toward reintegration into society. Patients can begin to take steps leading toward eventual productivity and independence even while they are still psychiatrically ill. In fact, it seems that such orderly progress may constitute another kind of treatment more appropriate for many patients than psychotherapy. Stotsky has been the most important contributor in this area. In one study (65) he shows that the majority of hospital patients rank psychotherapy very low when asked to make choices as to the value of twelve kinds of hospital activities. Stotsky & Weinberg (68) report on a sentence completion tests designed to measure nine aspects of ego-strength in schizophrenics and show that it predicts both whether patients will be below or above the mean in their response to various rehabilitation programs within the hospital and how satisfactory the whole treatment outcome will be six months later. Stotsky (67) has also constructed a special aptitude test battery designed to predict work performance and rehabilitation success. The tetrachoric correlation with work performance ratings in the hospital was .63 in one sample, .72 in another, and the tests predicted outcome of treatment in from 69 to 80 per cent of the cases. It is to be noted that the interest tests did not predict for this group. The battery that worked out so successfully was made up of subtests commonly found in general intelligence, mechanical aptitude, and clerical aptitude tests.

In another paper Stotsky (66) presents some findings as to motivation in schizophrenics. His hypothesis was that the impairment on psychomotor tests was motivational rather than organic in origin and could be counteracted by having encouragement and stimulation furnished by the patient's therapist during testing. The results supported the hypothesis to some degree. This kind of motivating influence did improve reaction time in all groups, but had less effect on the Purdue Pegboard. Both organic and motivational factors seem to be involved in complex tests.

Walter & Jones (77) discuss another sentence completion test similar to Stotsky's and show that it correlates moderately with ratings given patients in manual arts therapy. Klugman (37) presents some evidence that psychotics in remission show Kuder interest profiles not significantly different from those of Kuder's base group. Thus for such patients there is no reason to conclude that the interest pattern is invalid because of the mental illness, and it would seem to be legitimate to use the test as an aid to counseling.

Stubbins & Napoli (69) give a detailed case study describing what was involved in the counseling of one patient. The extra components that would not ordinarily be found in a nonhospital case are: a longer period spent in working through attitudes incapacitating the person for work, the use of hospital assignments as prevocational training, guidance in job-finding, and a longer than usual follow-up period.

Another extremely significant study related to the foregoing in conception and execution, although it was not carried out in a hospital, is reported

in the monograph by Gellman *et al.* (25). They give an account of a workshop set up by the Jewish Vocational Service in Chicago for clients who for various reasons are essentially unemployable. It offers an eight-week program of actual paid employment. Work is supervised by skilled vocational counselors who give each client the kind of treatment he needs in order to increase his employability. With a group of clients who by all standards would have been considered to have a poor prognosis, this project resulted in steady employment for 48 per cent of the cases. Only 21 per cent received no benefit. The change was in work habits and personal relationships on the job rather than in basic personality.

EVALUATION OF COUNSELING

Evaluation studies have not been numerous during the period of this review, if we omit the studies on the kinds of psychotherapy that do not constitute counseling as defined in our first section. Increasing methodological sophistication is apparently making research workers hesitant about reporting simple "before and after" studies. Borow (12) discusses the logic of counseling research and analyzes the kinds of work that must be done before over-all evaluation of outcomes can be meaningful. He gives high priority to an activity now in progress at the University of Minnesota, an inquiry into the language of personality description.

Cartwright & Roth (17) report a factor analysis of the outcome measures used in the Chicago study described in the Rogers & Dymond report (55). Three factors emerge from the posttherapy status correlations: client satisfaction, therapist view of success, and client response to others. Two factors appear in the analysis of measures of change. They seem to represent the client's view and the therapist's view, respectively.

Kaczowski & Rothney (35) explore the possibilities of discriminant analysis as a way of combining variables in studies where the counseled are to be compared with the noncounseled on a number of factors. They use data from the Wisconsin follow-up study of high school students to demonstrate the method. A brief report in the nature of a news item (86) says that Turney & Morehead have demonstrated that the presence of a trained counselor in a small high school gives its students an advantage in personality adjustment, academic achievement, and suitability of vocational choices.

Kirk (36) presents an evaluation of an in-service training course taken by ten high school counselors. The changes that occurred were in attitudes rather than in knowledge of practices and procedures. The shift was in the direction of more understanding and acceptance, more awareness of the complexity and responsibility of the counselor's task. The sentence completion test was the most satisfactory method of measuring change.

MISCELLANEOUS CONTRIBUTIONS

The special field of rehabilitation counseling is being rapidly expanded. Lofquist's new text (38) brings together general counseling principles and knowledge of the special problems presented by a number of disability

groups. Medical and psychological information has been skillfully integrated in these chapters.

The symposium report by Seidenfeld *et al.* (59) considers the factors that need to be taken into consideration in evaluating rehabilitation in the individual. It would seem to constitute a first step toward the specification of criteria other than employability in this rapidly expanding area of counseling activity.

The report by Hall *et al.* (30) on rehabilitation counselor preparation will be useful in the planning of training programs.

Semantics continues to be an important resource for some workers. Johnson (34) analyzes, with his usual clarity and charm, some of the relationships between personality and our use of language.

Williamson (80) again makes a case for disciplinary counseling, stressing its preventive aspects and the responsibility school personnel officers have of promoting moral and social growth.

There have been several reports on the characteristics of special groups with which counselors often must deal. Hewer (31) shows that successful and unsuccessful medical students at Minnesota are differentiated only by scholastic record and ability tests, not by Strong or MMPI scores. Patterson (47) summarizes 131 studies on trade and vocational school students, showing what are the best predictors of success in such work. Samuelson & Pearson (57) show that trade school dropouts are not differentiated from successful students in Kuder interest scores.

Miner's (44) detailed analysis of the relationship of various aspects of occupational status to intelligence, as measured by a brief, steeply graded vocabulary test, will be of interest to many because of the representativeness of the sample of the total population upon which it is based. One of the conclusions, that about two-thirds of the population of American workers are intellectually capable of functioning at a higher level of skill than the one at which they are now placed, has important counseling implications.

If counseling is defined or delimited as it has been in the beginning of this review, the basic theoretical ideas upon which it draws are those centering around the concept of the self. Thus the collection of papers in the volume edited by Moustakas (45) is of particular interest. They are by authors as far apart in culture and philosophy as Rabindranath Tagore and Jean-Paul Sartre. Many provocative ideas are presented.

The emphasis on choice and freedom is one which psychologists may find difficult to incorporate within a scientific framework. Thus many will be interested in the paper by Boring (11) in which he analyzes the issues centering around freedom vs. predetermination and concludes that we are justified in using both models at different times.

GENERAL COMMENT

The ideas that have been presented during the year have made it possible for counselors in different professional settings to become increasingly clear

about what their function is and how it can best be carried out. The way is open for the planning of research that will give us a better understanding of the process, the setting up of programs that will maximize its usefulness, and the elaboration of the aspects of personality theory to which it is related.

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ABNORMALITIES OF BEHAVIOR¹

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The development of a research field, like the development of an individual, often proceeds in cycles. There are periods of relative calm and stability, when controversies are stilled and research attention and energy are focussed on a few problems which seem crucial. But there are other periods of turbulence and restlessness, when the old conflicts recur and a wide range of problems and methods spreads research attention and energy into many channels. Research on abnormalities of behavior this year seems to represent the latter phase of development. The old functional-organic riddle is in the wind again, posed in new and sophisticated terms, but basically no more answerable than before. There is a special self-consciousness about formal matters of theory, hypothesis, and prediction: investigators select their problems and interpret their results in terms of theoretical orientations from many different areas of general psychology and psychiatry. There is a special concern over problems of research methodology and over the importance of appropriate statistical operations. There is an increasing distrust of conventional diagnostic categories, which has led workers to consider many diverse abnormal phenomena worthy of study in their own right, without reference to their status as symptoms or syndrome.

As in individual development so in the maturation of a research field, cultural history and personal past play a part. This was the Freud Centennial year; the mass of commemorative theoretical and experimental papers with psychoanalytic affiliations may therefore be an artefact rather than the expression of a stable trend. This was also the early-childhood period of research with LSD, chlorpromazine, reserpine, and other pharmacologic agents; the mass of physiologically-oriented research may therefore reflect only the initial enthusiasm which attends a promising innovation rather than a shift of research orientation. Time alone will tell. But for this year, these two events served to stir again the longings for an ultimate explanation of abnormal behavior, and hence to sharpen the functional-organic issue.

Both the selection and the organization of papers in this chapter follow from the foregoing considerations. From the hundreds of studies on human behavior abnormalities which employ acceptable research methods and depend upon appropriate statistical analysis, those which receive emphasis here are those which attempt to relate findings to the growing body of psychological and psychiatric theory and which show sensitivity to problems of methodology. Studies of significant abnormal phenomena are preferred over conventional diagnostic problems. These choices mean that a sizeable number of informative publications on many topics, particularly in the areas of mental deficiency, drug addiction, psychopathy, and con-

¹ The review surveys the literature from May, 1956, to May, 1957.

vulsive disorder, receive no mention. Such omissions are inevitable no matter what principle of selection is employed, but they are still regrettable.

THEORETICAL ORIENTATIONS

Today abnormal behavior is widely understood as based upon, derived from, and traceable to various aspects of everyday normal behavior. It is therefore not surprising to find that theories, hypotheses, and conceptualizations from many areas of general psychology seem applicable to behavior abnormalities. Efforts to relate findings to theory vary widely in sophistication, as do attempts at new theory construction or new conceptualization. Too many studies depend upon analogical reasoning, too few upon precise deduction. The demonstration of recurring errors in the construction of theoretical models in psychosomatic medicine, set forth by Mendelson, Hirsch & Webber (68), could profitably be undertaken for other branches of behavior pathology.

PSYCHOANALYSIS

Developments in theory.—The Freud Centennial stimulated the publication—and nostalgic republication—of many psychoanalytic papers. The two Freud commemorative volumes—Arlow's (4) summary of Freud's principal works and Jones' (51) four Centenary addresses—together provide a framework for understanding Freud's contributions and the cultural and personal factors which moulded them. Even as during his lifetime, however, so on the hundredth anniversary of his birth, Freud stands amid both critics and followers. This year the theoretical contributions of Adler, through the capable hands of the Ansbachers (2), have been made easily available for the first time. In three publications (80, 81, 82), Rado this year presents his divergences from Freud as expressed in his system of "adaptational psychodynamics." The Centenary year sees also the publication of the second volume of Abraham's collected works (1), including some early studies which seem to foreshadow Freud's later writings. The first English translation of Nunberg (73), on the other hand, expounds and supports Freudian theory as it applies to the neuroses, although much of the text seems, in these days of ego psychology, rather anachronistic.

Of the more recent contributions to psychoanalytic theory, mention can be made of only a few. The English school is represented by a memorial volume to Melanie Klein (54a). The third volume of Sullivan's lectures (100) contains Sullivan's concepts of dynamisms, as well as a great deal of significant theoretical and clinical material on various diagnostic groups, particularly schizophrenia. Hartmann (45a) offers a careful, if complex, analysis of the reality principle, beginning with Freud's early paper, by tying the concept more closely to ego functions. Two papers present additions to the understanding of depressive reactions. Lichtenberg (62), following the view of "hope" held by Lewin and by French, proposes a conceptual model of the depressions which is capable of objective test by level-of-aspiration

techniques and studies of small-group behavior. More comprehensive and less amenable to laboratory-type testing is the concept of the depressive constellation described by Benedek (10), which develops from a complex pattern of endocrine states and mother-infant gratification and frustration.

Experimental studies.—Perhaps the best example this year of the integration of psychoanalytic theory with research is the continuing series of papers on anxiety by Grinker (43) and Grinker *et al.* (44). A careful analysis of the concept of anxiety is presented, as are two reports of physiological reactions in anxiety states (7, 77). The Michael Reese group's adherence to a set of basic postulates, and its design of methods to test specific predictions, are welcome antidotes to the many loosely-conceived and inclusive studies which creep into this field.

There are, however, a few additional, less comprehensive studies which follow rather rigorously from various aspects of psychoanalytic theory. The place of expressed hostility in psychopathology is examined by Wirt (113) in an investigation of Rorschach content elicited from normal, neurotic, and schizophrenic patients. Following Freudian and neo-Freudian theory, Wirt hypothesizes that normal Ss will produce less hostile ideation than neurotics, and neurotics less than psychotics. The findings are contrary to the hypothesis: at least as it is measured by the admittedly limited technique of the Rorschach, hostile ideation is not related directly to severity of illness. Another angle of the Freudian view of hostility—its camouflaged expression through wit—forms the basis of Byrne's study (18), which also fails to support the psychoanalytic hypothesis. Neurotic patients whose typical manner of expressing hostility was known sorted hostile and nonhostile cartoons on the basis of how funny they seemed. Later they classified the cartoons as hostile and nonhostile. Contrary to predictions, patients who were rated as typically expressive of hostility found the hostile cartoons funnier than did nonhostile patients. Wirt chooses a Hullian rather than a Freudian interpretation of his findings. The same Freudian view that certain forms of speech—here negation—permit the verbalization of attitudes consciously rejected by the patient is employed by Laffal, Lenkoski & Ameen (59) in explaining an unusual symptom of "opposite speech" in a schizophrenic patient.

The nature of basic ego functions in abnormal states is the focus of a small group of studies, of which the one by Singer, Wilensky & McCraven (97) is a good example. Starting with the psychoanalytic hypothesis that capacity to delay is related to the development of thought, these writers performed a factorial analysis of a large battery of tests administered to 100 schizophrenic patients. From the four factors extracted, the authors conclude that the motility-fantasy relationship is a complex one, which may result in impulse-restraint and planfulness, but also in introspective withdrawal and reverie. Similar reasoning underlies another paper, by Singer & Opler (96), which yields evidence for differences in motor and fantasy behavior between schizophrenic patients of Irish and Italian ethnic background.

The balance between impulse and control in aggressive behavior is studied by Purcell (79) in an investigation of Thematic Apperception Test (TAT) protocols obtained from 57 Army trainees referred to a mental hygiene clinic. Overtly antisocial patients expressed more TAT aggression, more crudely and directly, than did less antisocial patients: but, contrary to many formulations of criminal and delinquent behavior, anticipated internal punishment, as expressed in TAT stories, seemed a more significant control of antisocial behavior than external punishment.

The nature of repression (33), as studied by means of the analogue of differential recall, emerges again as a complex pattern involving the intensity and quality of the stimulus and the personality of the patient, in a study by Klugman (56). He asked neurotic patients and controls to construct their own pleasant or unpleasant phrases from a word book, and to classify them as mild or strong in feeling tone. These phrases, paired with nonsense syllables, constituted the materials for initial learning and later recall and relearning. The principal psychoanalytically-oriented hypothesis—that both groups would retain more pleasant than unpleasant material—was not supported.

LEARNING

Conditioning.—The assumption that some sort of social learning determines the development of abnormal behavior underlies a great many studies to be summarized in later sections. A few papers this year, however, attempt to apply conditioned-response-type theories of learning to behavior pathology. Phillips, for example (78), depends on Miller's concepts of conflict, particularly the approach-avoidance type, in accounting for the difficult and confusing problem of childhood autism. Phillips uses the Miller formulation only analogically, arguing that the boy who develops autistic behavior is one who has learned over-assertiveness to the point where he characteristically expects too much from his environment, and therefore constantly meets overwhelming odds.

In a more sophisticated presentation, Wolpe (114) reviews some of the discrepant findings in the field of experimental neurosis, and argues convincingly for a conditioned-response explanation of this type of behavior. When he extends the argument to the learning of neurotic behavior by human Ss in a social environment, however, the result is less convincing. This pervasive limitation of the conditioning theories is pointed up in an attempt by King *et al.* (54) to evaluate the usefulness of Skinner's conceptualization of operant motor behavior in predicting both severity and clinical improvement in 30 acute schizophrenics. No linear relationship emerged between response-rate of pulling a plunger for reward, and either severity of illness or clinical improvement.

A fuller account of the relationships between operant behavior and psychopathology is presented by Skinner in his chapter in the Washington University symposium on the psychoses (106). As might be expected from

his other writings, Skinner argues for the careful study of observable behavior to the apparent exclusion of reactions, such as affective ones, which most workers with psychotic patients find essential. Few will quarrel with his insistence that psychotic behavior is lawful; but to many this general account of pathology will seem as unpromising as the foregoing application of the plunger-pulling technique.

The hypothesized motivational properties of clinically defined and experimentally induced anxiety have for some time been the subject of controversy in the field of learning theory. Rosenbaum (85), in a study of stimulus-generalization in psychiatric patients and controls, found the predicted positive relationship between high levels of anxiety and elevated gradients of generalization only under conditions of strong shock. His results support the interpretation of clinical anxiety as a drive state, but suggest that this drive is activated only in the presence of particular noxious cues.

Conditioning and learning techniques form the basis of treatment in an account by Jones (52) of the course of a psychiatric patient who suffered from "anxiety reaction with hysterical urinary frequency." In this procedure, manometer readings (either true or falsified) became the patient's cue to a felt urge to urinate. After five laboratory sessions, the patient reported feelings of ease and confidence, the adaptive contractions and relaxations of her bladder wall became more normal, and urinary symptoms disappeared. The remaining anxiety reactions yielded after two weeks of more general "re-education"; and the gains persisted over a 15-month period. The assignment of symptom change to the particular conditioning techniques will be disputed by many in view of other possible factors, say, the generally supportive attitudes incidental to the technique, or the discomfort of the procedures.

Social learning.—To many writers, a broad theoretical framework encompassing social learning has seemed more fruitful than the more rigorous theories derived from conditioned-response formulations. Although this framework provides a general orientation for research, its looseness and incompleteness prohibit it from being considered theory in any strictly logical sense. Collier's (26) theory of psychopathology may prove to be an exception to this generalization, although it includes biological as well as social conceptualizations. Collier continues this year his publications on consciousness as a regulatory field, dealing with his concepts of primary and secondary defense and their relationships to the occurrence of neurosis and psychosis. The validation of these formulations remains for the future; at present, they would seem to be more helpful in organizing old knowledge than in generating new hypotheses about abnormal behavior.

Early child-parent relationships assume primary significance in any view that abnormal behavior develops by way of social learning. The papers, discussions, and case histories presented to the international seminar sponsored by the World Federation of Mental Health and edited by Soddy (99) provide a somewhat selective account of our present knowledge concerning the relationship between infant development and mental health. The bulk of the

material on early parental influences found here—represented, for example, by Spitz, Bowlby, and Anna Freud—is already well known. The case presentations of infant development in diverse cultures may prove more enlightening than the more formal research studies.

A refreshing contrast to the accumulation of anecdotal reports, confusingly defined parental "rejections" and "overprotections," and theoretical speculations in this area is the paper by Kohn & Clausen (57) on the relationship between parental authority behavior and schizophrenia. The study is based upon systematic interviewing of a sample of schizophrenic patients from a single community and data on carefully matched controls. By contrast with the controls, significantly more patients perceived their mothers to have been strict, dominating, restrictive, the primary decision-makers in the family. The patients see their fathers as strict, sure of themselves, and restricting, but not as dominating. Guttman scaling techniques applied to the interview statements on authority yielded two negatively intercorrelated scales: one on strength of maternal authority behavior and one on parental authority behavior. Over-all group differences are complicated by significant sex differences (male schizophrenics' perceptions of their parents are closer to those of control women than of control men); and by significant effects of socioeconomic level. This study is probably more promising for its method than for its results; but it is even more promising in its candid recognition that the pattern of maternal dominance and dependency on the mother which emerges from these data on schizophrenic patients appears with monotonous regularity when many other illnesses (ulcer, anorexia, delinquency, addiction, depression) are studied with other methods. We are a long way from isolating a particular parent-child relationship which is specific to the development of a particular behavior abnormality. As in this study, so in others, early relationships with the father are beginning to receive the research attention once reserved exclusively to the mother.

The assumption that early social learning in the parental context determines the development of behavior pathology dictates also certain emphases in the treatment of abnormal states. In addition to the "relationship" and "uncovering" methods of psychotherapy² which are oriented toward early paternal attitudes (and are well exemplified by Gardner (40)) may be mentioned the "regressive" forms of treatment in use with psychotic patients. Secheyay's lectures to the staff of the Burgholzli Clinic have been published this year (92), and provide further systematic descriptions of her method of symbolic realization in the psychotherapy of schizophrenic patients. This approach, which involves a close, supportive relationship of therapist to patient, to the end of gratifying basic, infantile needs, has now begun to show its influence upon other therapists who deal with psychotic patients. Azema & Wittkower (6), for example, describe a variation of the Secheyay

² Studies on psychotherapy and counseling are reviewed on pp. 353-74 and 375-90 of this volume.

method, influenced also by Margolin's publications, in which severely ill schizophrenic patients are offered objects resembling those experienced by an infant: milk, nursing bottle, mud. These exact objects are gradually replaced by more symbolic representations (parties instead of nursing bottles). In the small group of patients studied, the method seemed to encourage the re-establishing of contact with reality, although much more evidence concerning the limitations of this approach is needed before any conclusions can be drawn.

Not only accounts of the etiology of illness but also explanations of certain phenomena in abnormal behavior depend upon socially oriented views. The social learning approach is particularly attractive to students of schizophrenic disorganization, who are understandably impressed by the profound social disarticulation which characterizes the schizophrenic patient. A general account of schizophrenic misperception of other persons, for example, is provided by Burnham (17), in terms of F. H. Allport's theory of event-structure.

The theory of social role-taking expounded by Mead and Cameron underlies Diamond's (30) study of the ability of schizophrenic patients to modify their judgments of the autokinetic phenomenon in the presence of a control subject, by contrast to their performance when alone. Although both schizophrenics and controls modified their judgments in the social situation, the controls made greater changes than did the schizophrenics. When the Ss were retested alone, schizophrenics deviated more from their pair-members' estimates than did the controls. Thus schizophrenic patients were less responsive to and less enduringly affected by an interpersonal situation.

The comprehension of social situations by schizophrenics is evaluated somewhat differently in a paper by Senf, Huston & Cohen (95). These writers presented cartoons to chronic and early schizophrenics, and to two control groups, on the assumption that the social difficulties of schizophrenic patients would be reflected in distortions of their comprehension of comic situations. Although all groups of Ss perceived the general environmental setting of the cartoons with equal accuracy, the chronic schizophrenic group had difficulty even in identifying the speaker, and seriously distorted the accounts of the action, the social roles, the motivation, and the humor in the pictures.

The rather specialized field of conceptual thinking in schizophrenia shows some social-learning orientation, although the reanalysis of Goldstein's views of abstract and concrete behavior is both more common and more productive. Whiteman (111), for example, seeks to relate the impairment of conceptual thinking in schizophrenia to social disarticulation by means of a sorting procedure. Schizophrenics were significantly inferior to controls in sorting pictured social situations, producing more individualistic and inappropriate responses, and rejecting more stimulus-materials. Even when the simple dichotomy of human-nonhuman stimuli is employed, schizophrenic problem-solving behavior is more disrupted with the human than with the nonhuman

stimulus-material, as shown by Davis & Harrington (29). So many other variables affect sorting behavior, however, that these results must be considered rather minor contributions.

Chapman (19, 20) and Chapman & Taylor (21), in a particularly well conceived series of studies, provide evidence that "concrete" and other incorrect responses of schizophrenics on a sorting test are at least in part a distraction phenomenon. Indeed, a "distracter" stimulus was often employed incorrectly as a basis for sorting even when such a response required conceptualization. Support is also given, in one of the papers, to the notion of an inappropriate broadening of the conceptual basis of sorting as the reason for sorting errors. These findings fit rather well Cameron's views of overinclusiveness, and of the intrusion of inappropriate stimuli, as factors in producing disorganization in schizophrenic thinking.

That these factors may not be restricted to the thinking of schizophrenic patients is suggested by a study reported by Payne & Hirst (74), employing the Epstein Overinclusion test with depressive and control Ss. In this study, depressive subjects overinclude more than controls and more than did the schizophrenic patients described by other investigators.

A further challenge to Goldstein's view is offered by McGaughran & Moran (66), who isolate four conceptual variables from the abstract-concrete formulation. Earlier, these writers showed that the relative amount of communality or social agreement for the concepts distinguished schizophrenic from normal Ss. In the present study they demonstrate that another conceptual variable—order of conceptual classification—significantly distinguishes schizophrenic from brain-damaged patients, with the brain-damaged employing a lower order of classification. Both the Chapman and the McGaughran investigations demonstrate the new insights which can come when older and more traditional formulations are reanalyzed and subjected to scrutiny by means of new techniques.

Schizophrenic disorganization, of course, is by no means the only phenomenon which invites interpretation based upon social factors. Indeed, symptoms which have long been considered physiological in origin are being reinterpreted in personal-social terms. To take just one example of a trend, Weinstein's book on denial of illness is followed this year by two papers further analyzing the reactions of patients with brain disease. Weinstein, Kahn & Malitz (109) point out the adaptive function which confabulation and other language distortions serve in patients with head injuries. The observation that these patients seem free of clinical anxiety during periods of confabulation and seem to develop agitation and depression when confabulation or other language distortions disappear, supports the interpretation of these language forms as a form of social behavior by which the patient adapts to stress. In another paper, Weinstein & Kahn (108) extend this reasoning to loss of insight and inhibition, and dulling of consciousness, in brain disease. These reactions also may be considered changed patterns of social interaction which assist the patient in adapting to the stress of ill-

ness and incapacity. The authors argue that none of these behavioral characteristics of brain-injured patients is explicable solely in terms of specific lesions or specific neurophysiological disabilities.

PERCEPTUAL BEHAVIOR

Theoretical interrelationships.—Recent emphasis on perceptual behavior and its theoretical formulation is reflected also in the field of behavior pathology. Certain parallels between clinical observations of neurotic behavior and Brunswik's probabilistic-functional theory of perception, and the views of the Ames group, are recorded by Imboden (49). Inappropriate neurotic behavior, for example, may stem from unveridical perceptions; and its persistence and repetition may follow the course of persistent misperceptions as described by Brunswik. The phenomenon of transference in psychotherapy may likewise be considered a misperception, in which the patient perceives his therapist in terms of a probability estimate regarding the significance of particular sensory cues. A similar attempt to apply perceptual principles of psychotic behavior is made by Scher (91), who provides a lengthy historical review of the literature on perception. His clinical examples of the applicability of these principles to the understanding of psychotic patients are provocative; but any systematic tie-in between behavior pathology and particular perceptual theories is missing.

Experimental studies.—A general demonstration that visual discrimination and motor reproduction, as measured by modified scoring of the Bender-Gestalt, decrease with severity of psychopathology is provided by Niebuhr & Cohen (72). The possibility that these findings are influenced also by age, contrary to previous results, cannot be overlooked. That impairment in perceptual accuracy may vary with the nature of the task is shown by the results of Cohen, Senf & Huston (24) in a study employing the Hands test, the Street incomplete figures, and selected Rorschach scores with schizophrenic, depressive, and neurotic patients.

It is, however, with brain-damaged patients that the study of perceptual behavior and adherence to perceptual theory are most common. In a carefully controlled comparison between veterans with penetrating brain wounds and veterans with peripheral nerve injury, Teuber & Weinstein (103) found a significant deficit in discovering the Gottschaldt hidden figures related to the presence of cerebral lesion. The absence of significant differences among subgroups of the experimental sample defined by locus of injury leads to the tentative conclusion that cerebral lesions in man have both specific and general effects. As in other perceptual studies, the appearance of behavioral deficit is in part a function of the tests applied.

The perception of apparent motion, often thought to be impaired in patients with brain damage, is studied in a number of papers, of which two may serve as examples. Saucer & Deabler (89), following the formulations of Goldstein, Kohler, and Wallach, presented two flashing light bulbs with varying rates of alternation to organic, schizophrenic, lobotomized, and

control Ss. Both organic and schizophrenic patients showed a lower threshold of dissociation of Wertheimer's beta motion than did the other two groups; but they were not significantly different from one another. While the interpretation of the organics' deficit as due to functional inefficiency of the cerebral cortex seems justifiable, the concomitant interpretation of schizophrenic deficit as due to psychological factors which may or may not have a physiological basis seems less helpful. The application of a new "cycling" tachistoscopic procedure to the study of apparent movement in brain damaged and control Ss leads Brenner (15) to conclude that earlier reported effects of stimulus-complexity and meaning upon threshold do not hold up. The hope of employing the threshold of apparent motion as a diagnostic indicator of brain damage is somewhat reduced by these methodological complications.

Similar caution in the use of perceptual distortion as diagnostic of brain damage is suggested by two illustrative studies of rotated figures. Williams *et al.* (112) found differential effects between brain-injured and control Ss on the Shapiro Block Design Rotation Test, but the results are by no means simple. The factors of figural symmetry, figure-ground congruence, field reduction, and intellectual level all contribute to the results. When Yates (115) employed drawings of block designs rather than manipulation of the blocks in a similar study, he found more rotation in brain damaged than in control patients. In the drawn version of this technique, however, symmetry was no longer a significant variable; and drawings of "twisted" or distorted designs within the outline of the figure sometimes were used by brain damaged patients. This finding fits in with the facilitating effect of a field reducer for the brain damaged patients in the study by Williams *et al.* Both hint at the importance of surrounding distractions in the performance of brain injured patients.

PHYSIOLOGICALLY ORIENTED VIEWS

No reader of the literature on abnormal behavior can fail to be impressed with the increasing time and energy being devoted to physiological studies. A number of factors contribute to this unmistakable trend. The successful induction of pathological-like states by drugs, and the apparent efficacy of the ataractic drugs in alleviating symptomatology—both discussed below—have generated great enthusiasm for further physiological investigation. The growing volume of research in the psychosomatic field, also reviewed later in this chapter, not only gives promise of greater understanding of the intricate interrelationships among human reactions at all levels, but points up the necessity for closer study of the physiological reactions as such. To workers in search of more precise investigative techniques than the rough behavior ratings by ward personnel or the often cloudy and confusing pictures yielded by projective instruments, physiological measures may be a welcome change.

Developments in theory.—To these general determinants must be added the influence of particular recent theories. The Grinker *et al.* view of anxiety (43, 44), mentioned earlier, assigns central importance to the physiological

aspects of stress in the infant organism, and a return to the global infantile state in the face of later stress, either psychological or physiological. Selye's (93, 94) influential concept of the General Adaptation Syndrome is another case in point. The productiveness of this concept in explaining abnormalities of behavior in physiological terms is demonstrated in a paper by Cowden (27), who equates Selye's end-stage of exhaustion with the clinical picture of the apathetic, withdrawn schizophrenic patient. Miller's construction of a behavior theory which generalizes many principles from the physiological field also has its applicability to behavior pathology, as shown not only in his writings (70), but also in a number of studies beginning to emanate from the Michigan behavioral science group. Collier's theory of psychopathology, mentioned earlier (26), while it emphasizes the development and breakdown of habitual defenses under stress, still defines consciousness—a central concept—partly in terms of metabolic activity in the physiological gradient field. Arieti (3) attempts to describe the possible functional alteration of the nervous system which would account for schizophrenic symptomatology; he concludes that the schizophrenic, in avoiding anxiety, makes a psychosomatic effort to return to a lower level of integration, which means that the higher centers hypofunction, and lower neuronal patterns are released.

Research studies.—As in the case of social learning orientation, so in the realm of physiological views, it is schizophrenic disorganization which becomes the proving ground. Fabing (34) provides a review of the literature in biological research in schizophrenia, particularly in the area of pharmacology. Of more general interest to psychologists is Brackbill's (13) careful review of studies of brain dysfunction in schizophrenia published over the last twenty-five years. Although he points out a number of methodological limitations of the studies cited and is quick to note inconsistencies in techniques and results, Brackbill still feels, at the close of this critical review, that "the evidence is sufficient to postulate the existence of organic brain disease in some, but not all, schizophrenic patients" (13, p. 223). A good many psychologists will find themselves—perhaps unexpectedly—agreeing with him.

One promising approach seems to be the more precise differential between the old category of process (classical) and reactive (acute) schizophrenia. The clinical similarities between process schizophrenics and organic patients have, of course, been pointed out in the past. To the body of literature on this distinction may now be added two studies. Brackbill & Fine (14), using Winder's method of selection of schizophrenic patients, studied Rorschach responses made by process schizophrenics, reactive schizophrenics, and organic patients. On the basis of Piotrowski's organic signs, organic patients and process schizophrenics do not differ significantly, while reactive schizophrenics are distinguished reliably from the other two groups. It is to be hoped that readers of these results will follow the authors' caution in concluding only that process schizophrenics react to a perceptual task in a manner similar to that of patients with organic involvement.

Becker (8) approaches the problem from a different point of view—that

process-reactive syndromes may be end points on a continuum of levels of personality organization. He uses Werner's developmental theories to score Rorschach and proverb responses on levels of personality differentiation and integration; and he finds lower genetic-level Rorschach scores among the process-like schizophrenics than among the reactive-like schizophrenics. Apparently the process-reactive dimension can be related to a dimension of regressive and immature thinking, without necessary commitment at this time to an hypothesis of organic involvement.

The peak of studies involving chemical assays of body products in schizophrenia appears to have passed; current investigations of this type are likely to be critical, methodological ones. The paper by Vestergaard, Leverett & Douglas (105) will provide one illustration of the trend. This study of the excretion of 17-keto-steroids in the urine of schizophrenic patients suggests that the spontaneous variability of 17-keto-steroids is greater in schizophrenics than in normals—a finding which may invalidate studies of this point in which prior determinations of spontaneous variation have not been made. These authors found the total daily excretion values of their patients to be within the limits for normal persons of comparable age.

More promising is the continuing success of the Funkenstein test for autonomic nervous system reactivity (38) in distinguishing between groups of patients. In a study by Sloane, Lewis, & Slater (98), schizophrenic, depressive, and neurotic patients were given epinephrine (for sympathetic nervous system stimulation) and mecholyl (for parasympathetic activation) on separate days, and their reactions determined on eleven different variables derived from the shape of their blood-pressure response curves. The depressives showed a characteristic sympathetic underreaction, the schizophrenics a sympathetic overreaction. The niceties of physiological measurement displayed in this study are rather overshadowed by the absence of any hypothesized physiological mechanism to account for the findings. Still, the consistent demonstration of differential responses to a cholinergic drug by diagnostic groups, as well as the predictive power of this test for the efficacy of certain physical therapies, argue strongly for the involvement of autonomic nervous system function in behavior abnormalities.

Physical therapies.—The trend toward dependence upon physiological explanations and techniques is shown most forcefully in the therapeutic methods which are described in this year's publications. Enthusiasm for the tranquilizing drugs as therapeutic agents in behavior abnormality remains at a high pitch. Although isolated statements of caution, reports of occasional complications, and a few thoughtful accounts of the philosophy underlying this symptomatic treatment have found their way into print, still the flood of publications describing the improvements produced by these drugs is unabated. Most of the papers, however, overlook such methodological niceties as control groups, reliability studies of behavior ratings, standard dosages, and comparable treatment periods among groups. For samples of the range of findings and of the attitudes toward them, the reader is

referred to the volumes by Gottlieb *et al.* (42) and by Kline (55), and to the paper by Szasz (102). Even the most enthusiastic drug-therapist may be made uneasy by Szasz's penetrating analysis of the philosophy which seems to underlie this most recent "furor therapeuticus."

Sensitivity to problems of methodology in this area is shown in an illustrative design of a study for the evaluation of drugs by Sainz, Bigelow & Barwise (86). The account by Rashkis & Smarr (83) of the first stage of their research to appraise the effect of four drugs on a chronic hospital population is even more helpful in anticipating the problems which may arise. These writers transferred their 48 female schizophrenic patients to a special residential research unit, where for a seven-month period a baseline was established against which drug-induced changes were to be evaluated. It will surprise few clinicians to find that 39 of the 48 patients showed measurable improvement in behavior during the waiting period. Similarly helpful is the paper by Trouton (104) which reviews the literature on placebos, covering the seldom-reported pharmacologic and physiological effects of inert substances in placebos, as well as the characteristics of the "placebo-reactor." Much time and energy would be saved if research workers in this area were familiar with the main points of these papers before they undertook to appraise the effects of drugs on patient behavior.

A fairly representative example of the studies demonstrating positive effects of ataractic drugs is the paper by McDonald, Ellsworth & Eniss (65). They administered reserpine to 13 chronic schizophrenic patients and placebos to 14, in the double-blind design where the assignment of Ss to drug and placebo groups is known neither to the patients nor the researchers. Ratings of behavior were made by means of scales constructed for use in occupational therapy, correctional therapy, and ward settings; the ratings were evaluated in terms of the standard error of the rater, who remained constant for any single patient. Ratings of the drug group showed significantly more improvement than ratings of the placebo group. As in many of these studies, the relatively small N and the questionable reliabilities of ratings decrease the dependability of the results.

In a more carefully conceived study by Pearl *et al.* (75), reserpine, placebo, and no medication were administered to three groups of schizophrenic patients totalling 170 Ss. Eleven factors of the Lorr scale were employed to evaluate behavior changes. Most of the chronic patients showed no significant improvement with reserpine; the less chronic and more disturbed patients improved in some of the factors. The extent of improvement was not, however, great enough to make the bulk of the patients eligible for early discharge.

In a similarly designed study, Arnold & Freeman (5) found improvement in socialization, as measured by the Malamud-Sands scale, in chronically disturbed patients who received reserpine. No group differences appeared until the dosage reached rather high levels, however; and there were wide individual variations in the amount and duration of the improvement. Fif-

teen weeks after medication had been stopped, the mean scores of all groups had returned to their original baselines.

Representative of the group of studies in which the effects of the ataractic drugs seem insignificant is the paper by Penman & Dredge (76). Here both medication and ward arrangements were varied with five groups of chronic schizophrenic patients over a six-month period. Weekly behavior ratings were obtained on the Fergus Falls scale, and Minnesota Multiphasic Personality Inventory profiles were obtained before and after therapy. Neither reserpine nor open ward privileges resulted in significant patient improvement.

In similar vein, Hauck, Phillips & Armstrong (46) studied groups of patients representing different degrees of illness who were given projective tests before and after a two-month period of reserpine medication. No significant change in test scores, clinical judgment of entire test batteries, or attendants' evaluations of patients emerges which could be assigned to the effects of drug therapy. Projective instruments showed no change, over the two-month period, in basic patterns of personal adjustment.

It would appear that the ataractic drugs stimulate social improvement of a mild but not dramatic sort in acutely disturbed patients, that these effects are dependent upon continuing medication and decline when the drug is withdrawn, and that the behavior changes are not reflections of a basic alteration in personality pattern. The mechanisms underlying the reported improvement are by no means agreed upon; both interpersonal and physiological factors are probably involved. To tranquilize a disturbed patient is to tranquilize those who work with him as well; the shift of interpersonal balance which ensues may itself be the effective therapeutic agent. This is not to ignore the demonstrated physiological changes which the ataractic drugs induce. Urinary alterations, tachycardia, allergic reactions, extrapyramidal syndromes, and many other undesirable but typically transient side reactions of the drugs provide evidence for the basic shift in physiological economy which occurs.

As chemotherapy becomes the current fad, other forms of physical therapy seem to be employed less. Most of this year's studies on surgical intervention represent a summing-up and retrospective evaluation of projects undertaken years ago, in the heyday of the scalpel. Studies of the relative effects of two types of topectomy procedures are presented in a series of papers edited by Lewis, Landis & King (61). Few of the wide range of studies reported here show a differentiation between the two varieties of operation; indeed, few show significant differentiations between operated and control patients. Similarly discouraging results are reported by Mermelstein (69) in a follow-up of 100 chronic schizophrenics who had been subjected to transorbital leucotomies. A sizeable proportion of patients who seemed improved shortly after surgery relapsed; 75 per cent of the total group was categorized, on the follow-up, as not improved, and only 9 per cent were considered moderately or greatly improved. Systematic testing of 15 lobotomized and 10 control patients on 106 measures prior to, and two weeks, three months, and one

year following prefrontal lobotomy, as reported by Winne & Scherer (110) yielded too few significant differences to be explained by other than chance factors.

Reports on electroshock are also infrequent this year. Two methods of delivering the shock stimulus, one requiring less current to produce a seizure than the other, are compared in a paper by Kendall, Mills & Thale (53). There is some evidence that the procedure using less current causes less post-shock impairment in a test of new learning, and less cognitive disturbance in the immediate post-shock period, although the latter difference disappears two weeks after the end of the course of therapy. A radical form of shock therapy, carried out daily until regression is induced in the patient, is reported by Glueck, Reiss & Bernard (41). Great improvement and no permanent organic brain damage are said to result from the application of this technique to 100 patients, mainly schizophrenic. Considerably more evidence is required before this rather extreme procedure is widely applied. The reactions of fear and anxiety which have often been considered part of the therapeutic effect of electroshock are examined systematically by Gallinek (39) in a study of 100 patients. Two-thirds of the patients expressed some fear of the procedure; qualitative differences in the anxieties reported seemed to be related in part to the structure of the patient's illness.

Psychosomatic interrelations.—Both the promise and the limitations of physiologically-oriented studies are pointed up by developments in the field of psychosomatic medicine. The general trend toward assuming that any bodily illness has its emotional components, and any personality disturbance its bodily ones, continues. "Psychosomatic illness" as a diagnosis is being replaced by the "psychosomatic approach" as a way of life. Coupled with this trend is another: a growing tendency toward interdisciplinary research on these problems, with expressed faith that human illness can be understood only by considering sociological, psychological, physiological, and chemical factors together, as these are studied by trained persons in the several fields. If the archaic functional-organic dichotomies are ever to be resolved, it is probably by such attitudes as these.

Two developments in method warrant primary consideration. One of the gravest difficulties in the way of appraising etiological factors in any sort of behavior abnormality is the dependence upon retrospective studies: only after the pathological reaction is identified does research attention focus upon the early events which may have led to the pathology. Such retrospective approaches, made necessary by practical exigencies, have many limitations: data gathering is bound to be influenced by adherence to particular hypotheses; distortions and falsifications of recall are inevitable; and control groups can rarely be either located or studied. Consequently, it is often said despairingly that only detailed longitudinal studies of enormous samples of the population could yield crucial information concerning the illnesses which appear as infrequently as, for example, ulcer or ulcerative colitis.

A beginning, in miniature fashion, to the resolution of this dilemma is made by Weiner *et al.* (107), who have for some time been investigating the contribution of serum pepsinogen levels to the development of duodenal ulcer. These workers made serum pepsinogen determinations of a sample of 2073 inductees; subgroups of Ss with extremely high and extremely low levels were studied with projective tests, the Cornell index, and x-ray investigation. According to the hypothesis entertained by these writers, Ss in the high serum pepsinogen group should be more likely to develop ulcer. Later x-ray studies of the two extreme groups—now in basic training—revealed the presence of ulcer in nine subjects, all of them in the upper 15 per cent of the total group on prior serum pepsinogen determination. Independent evaluation of the test data produced 20 criteria which differentiated hypersecretors from hyposecretors and showed major unresolved and persistent conflicts over dependency and oral gratification in those subjects who developed ulcer. The authors contend that three factors are necessary to the development of ulcer: high rate of gastric secretion, a particular psychodynamic constellation, and a noxious social situation.

A second methodological contribution stems from the body-image schema developed by Fisher & Cleveland (37) to account for a wide array of physiological measures. This schema, based upon Rorschach results, locates subjects in terms of their view of their bodies: some Ss see their bodies as barriers, others as permeable; and these sorts of body-image are presumed to be enduring aspects of a general personality pattern. As it applies to psychosomatic problems, the hypothesis predicts that Ss who develop psychosomatic symptoms involving the body exterior would emphasize the barrier quality of bodily boundaries more than those whose psychosomatic symptoms were interior. The schema proved useful in a study (36) of the site of cancer: patients suffering from breast cancer had a significantly greater tendency to conceive of the body as enclosed by an impenetrable boundary than did patients suffering from cervical cancer. Since a control group (ten years postcolostomy for cancer) did not differ in Rorschach indices from the cervical cancer group, the authors argue that enduring personality organizations rather than temporary effects of surgery or symptoms determine the body-image of the interior group.

The identification of personality "profiles" which presumably characterize the development of particular psychosomatic symptomatology continues, despite long controversy over both the assumptions underlying this hypothesis and the means employed to test it. A particularly careful study on this point is that of Moses, Daniels & Nickerson (71) on essential hypertension. Physiological measures, some made during psychoanalytic interviews, were made on patients who had suffered five years' continuous hypertensive states prior to treatment, and were followed for five years after remission. Parallels between blood pressure determinations and interview content suggested a relationship between elevated blood pressure and the mobilizing of excessive anxiety and rage in response to the deprivation of infantile dependent and security-status needs.

A study of Minnesota Multiphasic Personality Inventory and Rosenzweig scores obtained from anxiety, ulcer, hypertensive, and control Ss, reported by Lewinsohn (60), characterizes the ulcer and hypertensive group as more emotionally disturbed than the controls, although no support is given any particular etiological theory. Despite the hypothesized relationship between the inhibition of hostile impulses and persistently elevated blood pressure, the Rosenzweig scores yielded no significant differences among the groups.

Doubt is cast on the validity of the "profile" assumed to underlie the development of neurodermatoses in a study by Cleveland & Fisher (23). Projective tests and interview methods were employed in a study of carefully selected dermatitis patients compared with two control groups. Although the expected trends toward masochism, conflicts over paternal authority, and maternal rejection seemed to appear, the dermatitis patients were more preoccupied with hiding than with the more commonly postulated exhibitionistic trends; and they showed, upon interview, an unexpected concern over the mistreatment of the "underdog." It is clear from this sample of "profile" studies that the typical personality organization considered to predispose toward specific psychosomatic disorders is still far from validated.

The alternation of physical symptoms in psychosomatic disorder, and the alternation of psychosomatic reactions with psychotic reactions, have often been reported; additional evidence on this point is provided by Ehrenthell (31). Engel (32) suggests that the frequently observed alternation of headaches with bleeding in ulcerative colitis patients may be related to the patient's general personality trends. Headaches occur more often when the patient feels in control; bleeding occurs more often when the patient feels hopeless because of real or fantasied object loss. The deliberate therapeutic use of a physical symptom as a defense against severe psychosis is reported by Cowden & Brown (28); they were able to provide a severely ill schizophrenic patient with a new outlet for the expression of his difficulties which enabled him to make a satisfactory social adjustment in the community. How much of this symptomatic improvement was the consequence of the displacement to a physical symptom and how much the consequence of three months' special contact with the many staff members who participated in the "conditioning" to the pain is difficult to say.

SELF-THEORY

The "self-concept" as it is employed in contemporary research has roots in many different theoretical views. It owes something to Sullivan's writings, something to Freudian conceptions of the ego, something, by implication, to the formulations of Arieti, and much to the work of Rogers and his group. Three rather diverse papers illustrate recent studies of the self-concept in behavior pathology. Hillson & Worchel (48) tested certain predictions from self-theory and others from Adlerian theory in schizophrenic, neurotic, and control Ss who made self, ideal, and other ratings on the Self-Activity Inventory. With some reservations, the results indicate that the neurotic patient

depreciates himself and sees himself as ineffectual in meeting his needs. The schizophrenic patient, on the other hand, seems defensively to distort his self-appraisal and to lower his concept of the ideal self.

Further evidence of the role which this defensiveness may play in self-acceptance as measured by self-ratings is furnished by Zuckerman & Monashkin (117). These writers found significant negative correlations between certain MMPI scales and self-acceptance measures in a group of psychiatric patients, and a significant positive relationship between self-acceptance and the K scale. Since they obtained no significant correlation between self-acceptance and life adjustment as measured by a case-history rating, they conclude that defensive effects may attenuate the usefulness of self-acceptance measures as prognostic indicators.

In another study, the same authors, together with Baer (116), employ Arie'ti's hypothesis that adjustment should vary directly with acceptance of self, parents, and other people. Here the additional findings emerge that patient groups have less acceptance of mothers, fathers, and other people than do controls, and that—as would be expected from genetic views of social attitudes—measures of acceptance of parents correlate with acceptance of other people in all groups.

SPECIAL METHODOLOGICAL DEVELOPMENTS

Self-critical attitudes toward research methodology characterize an encouragingly large number of investigators in the abnormal field. Appropriate use of statistical devices is becoming common even in medical-clinical studies. A useful summary of the available experimental techniques in abnormal psychology is provided by Burdock & Zubin (16). These authors present a table of measurable activities as these are related to behavioral categories and stimulus-classes. Examples drawn from the literature illustrate how such a rationale can lead to new insights into pathological behavior. Although numerous minor contributions to method have been made this year, only three areas seem worthy of special consideration.

Induced behavior abnormalities.—The experimental induction of pathological behavior by means of drugs continues this year, although the emphasis seems to be shifting in the direction of more detailed studies of the induced state and more cautious interpretation of its "model" properties. Lysergic acid diethylamide (LSD) is still the most common agent employed. Salvatore & Hyde (87) provide a careful description of the progression of this syndrome induced by LSD in 18 normal Ss, evaluated by a trained observer throughout one experimental day. A steplike progression of reactions is noted, beginning with anxiety and somatic symptoms, proceeding through perceptual distortions and feelings of unreality, accompanied by "awayness," followed by partially successful attempts to master the situation which may produce some confusion. In this study, all symptoms showed a general decline after the sixth hour.

Paranoid reactions during LSD-induced states were studied by Bercel *et*

al. (11), on the assumption that the personality structure of the subject might be related to the type of drug-induced behavior he showed. Fourteen of 25 normal Ss developed ideas of persecution; but the personality makeup of these Ss furnished a less reliable index for predicting the paranoid reaction than did the occurrence of complex synesthesia as part of the LSD-syndrome. Rorschach results (12) obtained at the height of the drug reaction and again five weeks later provided some prognostic indicators of the occurrence of a psychotic-like reaction, but less precise indicators of the type of reaction which would develop.

Such findings as these call for caution in the facile equating of the LSD-syndrome with a specific psychotic reaction. MacDonald & Galvin (64), in a detailed comparison of LSD-induced symptoms and those characteristic of schizophrenia, deny not only that the hallucinogens induce a schizophrenic-like state, but also that the experimental states are identical in form to any known "natural" psychoses. Clark (22) raises the same doubts, and provides as well a critical review of the literature on LSD, pointing out the lack of control groups, the absence of systematic observational methods, and the possibility of bias when interviewer and observer are aware of the use of medication with the subject. Liebert, Wapner & Werner (63), studying the visual perception of verticality as an index of primitivization, find contradictory evidence of the effect of LSD upon this reaction. Schizophrenic and normal Ss under LSD show greater alteration in the effect of the starting position on apparent verticality, which is consonant with the primitivization hypothesis. The simple perception of verticality under LSD, however, does not support the contention that this drug has a primitivizing effect.

Four additional methods for inducing deviant behavior should be mentioned. Cohen, Silverman & Zuidema (25) employed a centrifuge to establish blackout levels, and during this procedure chastised their Ss for moving about in the cab of the apparatus. Post-blackout interviews which focussed upon feelings of aggression and anxiety permitted relatively accurate independent estimates of blackout level.

Grinker *et al.* (45) developed a technique for inducing anxiety in psychiatric patients by means of a stress interview, directed toward the patients' known severest psychological conflict, conducted in a nonreassuring atmosphere. These writers found that, even with large amounts of personal information about their subjects, they could not control the degree of induced anxiety or accurately grade the stress stimulus.

Heath (47) used a somewhat similar approach to study the effect of anxiety upon the intellectual performance of schizophrenic patients. He used test-content constructed around anxiety themes, and obtained for each patient his individual pattern of thresholds of anxiety response to these themes. The results suggest that severe anxiety interferes with intellectual performance, but they show also that a major determinant of response disorganization is the patient's individual threshold of anxiety response to the particular situation.

In a study of hypertensive and normotensive Ss, Schachter (90) subjected patients to experimental situations calculated to arouse severe fear, anger, and pain. The experimental procedures induced significantly greater blood pressure elevations in the hypertensive than in the normotensive groups; there was also a tendency for the hypertensives to express more fear and anger overtly during the experimental periods than did the controls. Some side evidence for physiological specificity of emotional behavior was also obtained: epinephrine-like responses occurred with fear stimuli, norepinephrine with pain. Certainly the experimental induction of abnormal behavior in human Ss, whether by drugs or by social interaction, is at present unpredictable and incompletely understood.

Psychoanalysis as a research method.—The varied but orderly reactions which are presumed to occur during the course of psychoanalytic interview therapy have been the subject of much controversy. The impact of analyst upon analysand, the suggestive effect of interpretation, the complexities of transference and countertransference, the details of timing, the minimal cues inherent in word choice and abortive gesture, the "freedom" of free association, have been considered both challenges to and restrictions upon research on the psychoanalytic process. Because most of the psychoanalytic hypotheses about abnormal behavior derive from psychoanalytic interviews and are in turn circularly "validated" in interview situations, the question of the appropriateness of the psychoanalytic interview as a research tool is crucial.

Kubie (58) this year proposes the psychoanalytic interview as a model experimental procedure, which provides a "constant" observational situation. So long as the analyst can distinguish between his role as therapist and his role as scientist, he is able to keep the variations within his patient relatively free from contamination by variations arising within himself. A tentative interpretation may then be considered the deliberate introduction of a variable, whose "validity" is determined by means of predictions—to later patterns of free association, to subsequent emotional reactions of the patient within analysis, to symptom development, or to behavior outside the analytic hour. The most urgent problem, Kubie argues, is the study of changes in free associations which follow the introduction of interpretative material. This idealized picture will do little to dispel the doubts which the confirmed skeptic has about the psychoanalytic interview; but for many other students of abnormal behavior it will be welcomed as one more sign of increasing concern among psychoanalysts over research methodology.

It is not easy to translate this philosophy into an experimental design. Perhaps the Menninger Clinic's projected research program (88) will help solve the many practical problems involved. Meanwhile, it is far more common for the psychoanalytic interview to serve simply as a source of hypotheses, loosely confirmed by the recurrence of the same phenomena in several interviews, or by the reported experiences of other analysts.

An illustration of the more common use of the psychoanalytic interview in research is to be found in the Mayo Clinic studies of schizophrenia (9, 50).

Collaborative therapy was undertaken with 27 young schizophrenic patients and their parents, with each patient assigned to a different therapist. Interview material, initially directed toward the patient's life history, was discussed in group meetings of the therapists. From these discussions, a consistent picture of the genesis of schizophrenia seemed to emerge: the patient suffered early and recurring assaults—either psychological or physical—from his parent, to which he responded by introjecting the hostile aggressor. Provocative and correct as these formulations might prove to be, the method of their derivation falls far short of Kubie's goal. The earlier finding by this group of similar relationships between poorly integrated impulses in the parent and a wide variety of deviant child behavior (school phobia, fixations, antisocial reactions, perversions) could be interpreted either as validation of the formulation or as overgeneralization of a loosely defined concept.

A more precise, if more circumscribed, use of the analytic interview as a research tool is to be found in the paper of Moses, Daniels & Nickerson (71), described earlier, in which blood pressure determinations made during psychoanalytic interviews led to the establishment of a relationship between the mobilization of anxiety and rage over frustrated dependency, and excessive blood pressure elevations.

According to Kubie (58), psychoanalysis may make a second contribution to research methodology. In emphasizing the unfortunate interplay of neurotic need and distortion of research he says (58, p. 135),

... the most important contribution which psychoanalysis has to offer to science in general and to knowledge of the behavioral sciences in particular is the opportunity for the scientist to know himself in depth before he starts projecting his own confusion on the material which he is setting out to study.

Few will disagree with the importance of impartiality implicit in this statement, but few will agree that a personal psychoanalysis is the only means to this end, or, perhaps, that it is even an effective one.

That the preferences of the investigator may dictate choice of problem and method and influence interpretation of results is, however, incontrovertible. One study from the field of ataractic drug research illustrates the problem dramatically. Feldman (35) categorized 37 physicians who volunteered to participate in an investigation of chlorpromazine on the basis of their attitudes toward the drug, ranging from enthusiastic acceptance to complete rejection. At the close of the chlorpromazine trials, he discovered a distinct relationship between degree of success with the drug, determined by patient improvement, and physician attitude toward the drug. Since patient improvement was evaluated by a number of indices in addition to physicians' ratings, this result was not the simple effect of biased judgment. Subtle conscious and unconscious factors may apparently determine the efficacy of drug therapy. The operation of comparable personal factors in choice of certain diagnostic categories is discussed by Szasz (101) in a paper on malin-

Epidemiology.—The Yale epidemiological studies of prevalence of mental health and illness deserve special mention as methodological contributions. Summaries of these carefully conceived and painstakingly executed investigations are available this year in a number of sources, including Redlich's chapter (106) and the findings relative to psychosomatic disorder presented by Rennie & Srole (84). A sample of 1900 Ss, drawn from a populous residential area of New York City having a wide range of economic levels, was interviewed during a home visit. General relationships between socioeconomic status and prevalence of mental illness emerged. In the psychosomatic study, for example, arthritis and hypertension were less frequent in the high than the low economic brackets; colitis and skin eruptions had the reverse pattern. U-shaped frequency distributions characterized the prevalence of bladder difficulties and asthma; a bell-shaped distribution characterized heart disease. Indicators of tension and anxiety were not correlated with socioeconomic status, although they were highly correlated with multiple somatic ailments.

Like most population studies, the Yale studies are criticizable on many grounds. The mass approach, with limited interview time, can never yield enough significant material to answer all the questions which arise. Even the most careful sampling methods can be weakened by fortuitous circumstances: Ss are unavailable or noncooperative; classificatory schemes turn out to be less useful in practical than in conceptual terms. Gross relationships which emerge may seem trivial in view of the time and effort required to establish them. Still the very magnitude of the Yale effort ensures its influence as a stimulus to further methodological advance in this area.

CONCLUSION

In human development, a period of conflict, diffuseness, and instability often ushers in a time of reorganization and integration on a more mature level. Possibly, therefore, the controversy, contradiction, and diversity which seem to characterize this year's publications on abnormal behavior promise a new synthesis to come. Certainly there are hopeful signs in the increasing precision of methods used, in the willingness of investigators to look critically at long-cherished notions, in the testing of hypotheses from many varieties of psychological theory, and in the interdisciplinary attempts to understand the reactions of patients at many different interrelated levels. Whether the promise implicit in these trends is realized will depend upon the resolution of the rather basic issues inherent in three current developments: (a) The premature use of inexact models—whether they be psychoanalytic interviews or LSD-induced "psychoses"—to explain abnormal behavior. (b) The uncritical acceptance of a pragmatically successful therapy—whether it be symbolic realization or reserpine—as evidence for the etiology of the illness it relieves. (c) The choice of level of conceptualization and explanation—whether it be brain metabolism or introjected parental image—on the basis of a private value system rather than in terms of its appropriateness to the

behavior studied. The underlying issues are not peculiar to abnormal psychology; and they are not easy to resolve. The solution lies not in more research, but in research that is more rigorous, more penetrating, and more dispassionate.

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SOCIAL PSYCHOLOGY AND GROUP PROCESSES¹

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The tradition has developed, in the eight years since the first volume of this series, that a review such as this begins with some Olympian pronouncements. The forces responsible for the practice are unclear. It may serve as a self-administered reward for time and effort; it may be that the platform is so attractive that one cannot resist using it. Whatever the reason, before proceeding to the literature I propose to make three pronouncements.

The first pronouncement has to do with the quality of work being done in this area. In making it, I assume that the work reported in the major journals is representative of the general level of research being done by the profession. This assumption seems reasonable; editors of these journals use standards which are acceptable to the profession itself. On the basis of a long look at the research efforts reported in one calendar year, I have concluded that the quality of research is high. Major arguments among experimenters would probably take place over matters of judgments and the compromises that one is obliged to make if research is actually done, rather than over matters of logic, theory, analysis procedures, or research design. Undoubtedly experts in quantitative methods can find horrible examples to which to send their students in this year's collection of studies. It is becoming increasingly hard, nevertheless, to find studies in which the author unknowingly violates an assumption which the statistic he uses calls for. When violence is done, the investigator typically indicates that he knows his crime and reports slight feelings of guilt. Interesting and significant problems are being tackled intelligently and knowledgeably. One not infrequently hears deprecatory comments about the professional literature; I can only conclude these must rest upon a restricted area of interest or superficial analysis. As a collection, the year's efforts contain no sensational advances but do consist of many earnest and intelligent studies that, in the aggregate, make an appreciable advance in our knowledge.

Two pronouncements consist of recommendations to the god of research progress in social psychology for the coming years. The first is that the field sorely needs synthesizing, organizing articles. Our research efforts consist primarily of relatively small studies on small aspects of large problems. There is nothing wrong with this by itself. These are the kinds of studies which can be managed by the people who are doing most of our research these days: honors students, Master's students, Ph.D. candidates, and busy teachers

¹ This review covers the period between April of 1956 and April of 1957.

² The author gratefully acknowledges the assistance of Richard D. Mann, Sidney Perloe, and Joyce Talen in the preparation of this review.

supervising these people. What is needed are more and more of the integrating, summarizing papers of the sort which the *Psychological Bulletin* has provided so successfully in the past years. The second recommendation is calculated to make the task of the writers of such papers in the future easier and their products more useful. It is that each member of the scientific community be enjoined (by his conscience, of course) to replicate some part at least of previous studies. One is forced to entertain the proposition that some fifth-column spooks are around who nudge even those who start out to replicate a previous study into doing a completely new one. Rarely does one study build upon another; the many studies on a particular subject consist rather of independent attempts to explore the ground.

PERSONAL ATTRIBUTES AND SOCIAL BEHAVIOR

Included in this section are references to articles particularly concerned with the relationship between personal attributes and social behavior. Studies of this sort are of two sorts primarily: those which attempt to predict social behavior from characteristics of the person, and those which attempt to determine the personal correlates of social variables. The major emphasis in this section is on studies of the first type.

A careful search of the literature will, I think, support the proposition that social psychologists have not been notably successful at predicting social behavior from measures of personality. The reasons for this are many. Social behavior is, typically, multiply determined and the amount of variance contributed by single characteristics of a single individual is likely to be small. Social phenomena present complex problems of interaction. Another source of difficulty involves the nature of the predictive instruments themselves. By and large the measures of personality most frequently used come from clinical psychology which is quite appropriately concerned with inner states and well concealed needs. Clinicians have quite properly been trained to be suspicious of what the person says or does; these are often misleading as signs of internal states or basic motives. Unfortunately these acts—what the person says and does—are precisely the concern of the social psychologist. This difference in emphasis has led to some notable failures to predict social behavior from instruments which are perhaps useful in clinical practice.

Another instance of legitimate differences in approach which lead to the development of methods suitable for one activity but not successful for another is the treatment of personality in terms of its impact on others. The conception of personality as stimulus is almost categorically regarded as useless in respectable personality theories. Yet the stimulus value of an individual is of great importance to the social psychologist. The problem of predicting social behavior is further complicated by an inappropriateness or lack of fit between the social behavior to be predicted and the aspect of personality defined by the measure. The behavioral referents of a measure of personality structure are usually unclearly specified. This is, of course, not

true when the measure begins with behavioral referents as is the case with traits such as sociability and friendliness. Not many are of this sort, however. Even such an apparently behavioral trait as aggressiveness turns out to have many obscure and unclear behavioral manifestations.

Central to all of these problems is the need for the development of terms for describing the social situation. Jessor's (60) penetrating evaluation of criticisms made of phenomenological orientations in personality theory is relevant to these issues. Jessor demonstrates, to this writer's satisfaction at any rate, that there is no "fundamental logical or methodological distinction between S-R and R-R or phenomenological theories" (p. 177). This is not to say that there is no important difference between the two approaches: the significant difference occurs with respect to the data language. The S-R theory has emphasized and used effectively the data terms of physics, while the phenomenal theories have used more psychological language. This difference has given the former an appearance of greater independence in the definition of stimulus. Jessor argues that there is no objection to the method involved in the use of physical terminology but that the actual language of physics is quite inappropriate for describing social situations. Its undeniable success for the S-R theorists has been due to the fact that they have been concerned primarily with the behavior of the lower organisms. Having demonstrated the validity and appropriateness of psychological language, Jessor argues that one of the pressing tasks of psychology is to develop adequate terminology for the description of the environment. For no group of psychologists is the need more pressing than for social psychologists. In addition to the problems of prediction attributable to the difficulties in the personality measures, there is the very real problem of specifying meaningful dimensions of the situation in which the behavior takes place.

Relevant to these observations is the study by Cline (35) on the influence of social context on the perception of faces. His experiment has this history: Heider & Simmel (53) have demonstrated that people see, with a great deal of regularity, complex social relationships such as anger, fear, and intended attack in the movements of a circle, a large triangle, and a small triangle in an animated cartoon. This is taken by Cline to mean that essential dimensions of some social stimuli can be transposed into nonhuman patterns but remain perceptually equivalent. In the Heider-Simmel case, it is the motion feature of the stimulus which carries social data; the surrounding social situation can do the same. Cline was primarily interested in the changes in perceived properties of the individual which are attributable to changes in the pattern of group interaction. He proposes that a social group *qua* group is a part of an observer's (subject's) phenomenal field and this phenomenal field is co-ordinated to the interaction of the members of a social organization rather than to their individual actions per se. Outline drawings of faces were presented to subjects in pairs. Although it was possible to perceive noninteraction between the faces, subjects in fact perceived interaction consistently. There is clear evidence in the study that observers respond to a psycho-

logically extended stimulus field which includes the interaction of elements in the field and, most important for the present discussion, these interaction dimensions can be specified. This conceptual scheme and the methodology used by Cline can be fruitfully applied, as he suggests, to so-called ambiguous pictures of social situations used in projective tests. They can also be applied to the description of social situations used in group experiments.

The problems of predicting social behavior from personal attributes may be further reduced if a single theoretical system dictates both the personal characteristics and the critical dimensions of the social situation. Such an integrated approach is represented in the work of Cervin (28, 29, 30) who is conducting a series of experiments which derive from a generalized statement of Hull's behavior theory. He is attempting in a rigorous and, at the same time, inventive way to conceptualize both personality variables and dimensions of social situations in terms of Hullian concepts. The results of several experiments testing derivations from the theory indicate that this systematic attempt to build a theory of social behavior has a good deal of promise. In one study, for example, students with high emotional stability (conceptualized as high initial reaction potential) showed greater rigidity of opinion and greater dispersion of response latency in an opposition situation (conceptualized as non-reinforcement of the dominant response) (28). Further work with this approach calls for extensive testing of the validity of the basic assumptions of the system, a task which Cervin is pursuing (29, 30).

Personality traits and motive measures.—In recent years a number of investigations have deliberately set out to measure important social motives (4, 99). This effort has had from the outset an anchor in specific items of social behavior; the conceptualization of the motive began with a careful look at social performance. In this tradition is Veroff's (110) development of a projective measure of power motivation. Following the model used with the achievement motive (78), stories written by candidates for student offices just prior to the tallying of election returns were compared with stories written by students under less aroused conditions. A coding system similar to those which have been found useful in connection with the achievement and the affiliation motive differentiated between the two groups.

Interpreting correlates of newly developed measures of motives is always a delicate business. To a certain extent such data bear on the validity of the measure; in another sense, they may be regarded as illuminating the nature of the motive. Perhaps the wise procedure is to maintain both positions, recognizing the tentativeness of the development of both the concept and the measure. Veroff's results illustrate the nature of this problem. Persons with high need power scores, as compared with low need power scores, tended to have lower scores on the Social Value dimension of the Allport-Vernon Scale of Values, and were rated by their instructors as significantly higher in the frequency with which they tried to convince classmates of their point of view. These results are consistent with power defined as concern about controlling the means by which people can be influenced and, as such, increase confidence

in the validity of the measure. There are other findings, however, which appear to illuminate more clearly the nature of power motivation. For example, these high *n* power people, when asked to evaluate the satisfactions that would accrue to various jobs such as recognition, interesting work, self expression, high pay, did not differ significantly from low *n* power subjects in their rating of being boss. They also evaluated recognition from colleagues more highly than did the low *n* power subjects. These results, Veroff suggests, may indicate that "being boss" is not prestigious for this group and that need power has a strong component of recognition motivation.

There were a considerable number of investigations directed toward determining the social behavior correlates of motive measures. French (45), continuing her intensive and careful experimental program, reports a study in which the choices the subjects made of a work-partner were correlated with their need affiliation and need achievement scores. The predictions were that subjects with high need achievement and low affiliation motivation would choose a competent non-friend while those with high affiliation and low achievement motivation would pick a less competent friend for a work partner. These hypotheses were confirmed. This study illustrates the value of taking into account the relative strength of motives in making predictions of social behavior. It is true that at the present stage of methodology, some hazardous assumptions must be accepted before one can label one motive as stronger than another in a given individual. This seems, nevertheless, to be a preferable advance over the obviously too-simple idea that complex social behavior is determined by a single motive or the intensity of a single attitude.

The notion of simultaneously clarifying the conception of a motive and improving its measurement is also illustrated in this series of studies. Shipley & Veroff (99), in developing a measure of need for affiliation, defined the motive primarily in terms of separation anxiety and concern over rejection. Atkinson, Heyns & Veroff (4) in a very similar investigation arrived at a more positive conception of affiliation and defined the motive as concern with establishment, maintenance, or restoration of a close personal relationship, best symbolized by the word friendship. French & Chadwick (46) now report results which are consistent with this more positive conception of affiliation motivation.

A study by Atkinson & Raphelson (5) is particularly instructive on the general problem of predicting behavior from personality measures. The study makes quite clear that a particular motive is an important determiner of behavior only under conditions in which the behavior pattern is perceived to be instrumental to satisfying the motive. This conceptualization calls for much more careful specification of the situation in which the behavior takes place and its meaning in that situation as perceived by the performer than is usually provided. Specifically, these experimenters demonstrated that strength of the achievement motive is positively related to recall of

interrupted tasks when the situation was structured as one in which performance was instrumental to achievement. When this was not the case, the correlation between *n/achievement* and recall was missing. In a second experiment, need for affiliation was significantly related to recall of interrupted tasks when the situation was such that the performance was not related to achievement. When achievement-oriented instructions were given, the predictive efficiency of need affiliation disappeared.

Sometimes it happens that a personality characteristic fails to relate to social behavior, not because the characteristic is irrelevant, but because its influence is impeded by other factors. That role structure can restrict the impact of personality characteristics is illustrated in a study by Berkowitz (14). He compares the behavior of high and low ascendant subjects in a group problem-solving situation. Differences between the two types show up only in the early stages; they disappear in time as the influence of position in the group asserts itself and is adapted to. Those instances in which behavioral differences between high and low ascendant subjects persisted over the whole period were most likely to occur when the situational or role requirements were at a minimum. This study emphasizes that there are conditions under which role prescriptions override the influence of personality characteristics. Just how frequently this is the case is another matter.

Political behavior.—Most studies of political behavior and attitudes have emphasized the influence of such factors as social position, class, and group membership. Rosenberg (93) has explored the possibility that one's political ideology and behavior may have a strong relationship to one's basic ideas about human nature. Both political theory and operative political systems involve assumptions about the nature of mankind. A Guttman scale was used to order college students in terms of their faith in people. The items asked the respondents to indicate their notions as to the trustworthiness, cooperativeness, helpfulness, and concern for others. Students with low faith in people were more likely (a) to doubt the general public's qualification for voting on complex issues, (b) to perceive public officials as unresponsive to the will of the people, (c) to believe that candidates for office are not men of integrity, (d) to doubt the feasibility of democracy, and (e) to restrict freedom of speech. Faith in people was not related to party membership nor to liberalism and conservatism as such. The study demonstrates quite successfully that it is useful to approach political behavior by way of general attitudes, such as faith in people. As Rosenberg suggests, the faith-in-people variable may also be relevant to nonpolitical attitudes and behavior. It has been observed, for example, that the possessor of an authoritarian personality has a relatively low opinion of man (2, pp. 148, 154).

A study the results of which must be fitted in with those above is reported by Nettler & Huffman (81). Their problem was the relationship of political opinion to personal security as measured by the Maslow inventory. The results are clear; the more secure subjects were more conservative; the radicals were less secure. The major exception to this trend comes from the radicals

who were active in political organizations; these have high security scores. The other radical is a marginal man, with no group affiliation and with marked insecurity.

Social mobility.—Undoubtedly there are many factors at work to account for the fact that upward mobility is greater among members of the middle class than among members of the lower class. Differential accessibility of the necessary resources for mobility such as money and specialized training is certainly one factor. Sewell, Haller & Straus (97) report that the relationship between social class and vocational and educational aspiration cannot be attributed entirely to differences in intelligence, although intelligence is related to both. Roughly the same conclusion is reached by Pihlblad & Gregory (83). While not denying that there are general situational forces at work, Rosen (92) has explored the possibility that differential mobility is a function of class-related personality characteristics. Using male students in two large New Haven high schools, he tests the propositions that social classes differ in the strength of the achievement motive and in the value orientations which define and implement achievement motivated behavior. The results indicate clearly that youngsters from the upper social strata have higher need achievement scores. The same relationship holds with respect to values; the upper class groups show a high proportion who indicate future-oriented, individualistic, activistic value orientations. The motive score is significantly related to school grades; the value orientation is significantly related to aspiration to go to college. This study, indicating these differences in values and motives which are in turn related to educational achievement and occupational aspiration which are themselves related to social mobility, illustrates the complex relationship between social variables and personality traits. It represents an effective treatment of the complexity as well. A study by Empey (42) suggests that theory and research in this area can be made more precise if one distinguishes between absolute and relative occupational aspirations. It turns out that lower class youngsters have aspirations higher than the occupational level of their parents. It is not the case that they have restricted horizons although their aspirations are lower absolutely than those of students from middle and upper class background.

Dynes, Clarke & Dinitz (41) carry this concern with mobility, occupational aspiration, and personality traits one step further. They report a study of the influence of early parent-child relationships on occupational aspiration, as measured by a scale developed by Reissman (86). This scale was administered to 350 university students. An index of parent-child interaction was also obtained on these students. The data reveal that those with high occupational aspiration more frequently reported unsatisfactory interpersonal relations in the family than did subjects with lower aspirations. These differences showed up on such items as feeling of rejection, parental favoritism, childhood happiness, and attachment to parents. These findings are consistent with the findings of McClelland *et al.* that high need achievers

are more critical of their parents than are those with low need for achievement (78, p. 279). However, one should be cautious about assuming that these people with high aspiration levels (who are also, if other results are to be taken seriously, high need achievers) have, indeed, had more unsatisfactory relationships with parents than other people have. There is the possibility that these subjects are more willing to make derogatory comments about their parents and are generally less inclined to idealize the home situation. The negative relationships which have been found between need achievement and F-scale scores would support this interpretation (78, p. 286).

Social variables and personality.—Another type of study of the relationship between personality characteristics and social variables consists in comparing personality attributes of groups classified on the basis of social characteristics. One frequently used classification scheme is the urban-rural distinction. Subjects with urban backgrounds have been reported to differ from those with rural backgrounds in personality adjustment (76). Burchinal, Hawkes & Gardner (23) have collected data from three separate studies to test the usefulness of the urban-rural dichotomy in predicting adjustment. The data fail to support the earlier findings of Mangus (76) that farm children achieve a more satisfactory adjustment than city children. The authors suggest that, since personality is a joint product of the interaction between original endowment and the social environment, with the increasing similarity between urban and rural families it is not likely that children from these homes will differ in their adjustment. If any differences persist it will show up in comparisons between rural children and children from very large cities. It would appear that the rural-urban dichotomy is losing much of its predictive value.

In this tradition are the many studies that relate personality adjustment scores to social class or status. As Auld (6) has pointed out, many of these investigations suffer from severe design limitations. Sewell & Haller (96) report a very careful attempt to establish the design conditions necessary to determine the effect of social class on personality adjustment. Their results indicate a significantly positive relationship between personality test scores and two social status indices: father's occupational rating and a prestige rating of the child's family in the community. The relationship remained significant when the influence of other possible personality-rated variables was controlled. The relationship, although statistically significant, is very low; social status variables account for little of the variation in adjustment. The authors suggest, however, that the correlation figure may underestimate the actual relationship due to homogeneity of the sample and the crudeness of the indices of social status and adjustment.

ATTITUDES, ATTITUDE CHANGE, AND AUTHORITARIANISM

Attitude change.—Lieberman (73), in his study of the effects of changes in roles on attitudes, points out that a central conception in role theory has been

that one's attitudes are influenced by one's position in the social system. The frequent demonstrations of a relationship between roles and attitudes do not, however, indicate the direction of the relationship. Do officers have more favorable attitudes toward the army than enlisted men because of the roles they occupy or is it the case that people with pro-army attitudes are more likely to be selected to be officers? Lieberman has attempted to untangle cause and effect in a longitudinal study of workers, foremen, and stewards in a manufacturing concern. The results showed that groups consisting of men who had moved into positions of foreman and steward became more promanagement in attitude after they were placed in their new positions. Groups of former foremen and stewards who reverted to rank-and-file workers reverted to attitudes they had held previously as workers. The data clearly indicate that a change in role brings about a corresponding change in attitude. The mechanisms responsible for this process remain to be discovered.

Role playing has been demonstrated to influence an individual's private opinion (57). Following up this finding King & Janis (67) ask whether the augmented influence of role playing is attributable to the satisfactions provided by the role performance or to the influence of the substantive arguments made up during the role playing. In this latter effect, which the authors call "improvisation," the person convinces himself of the validity of the point of view he is called upon to defend. In a design in which opportunities for satisfaction and improvisation were varied, the results show clear support for the improvisation hypothesis. The amount of opinion change is clearly related to the amount of improvisation required but not to the amount of satisfaction provided by the role playing. King & Janis suggest several mechanisms to account for the results, among them the possibility that the improvisation required in role playing reduces the normal resistances to the acceptance of persuasive messages. Doubts about the communicators' trustworthiness, thoughts about opposing arguments and conflicting expectations concerning the consequence of adopting the position being advocated—these and other sources of resistance may be reduced by role playing.

Tannenbaum (107), in reporting a study of the influence on attitude change of the original attitude position and of the attitude toward the source of a communication, observes that the proposition that strongly held attitudes are less susceptible to change than weakly held attitudes is often stated but has not been systematically tested. He suggests that the principle should apply to changes both in attitudes toward a concept and in attitudes toward the source of communication. He used Osgood's semantic differential technique and regarded the position of a concept or the source on the evaluative dimension as a measure of attitude. Tannenbaum studied the influence on attitude change of original attitude position and attitude toward source as well as the interaction between them. Both turn out to be significantly related to amount of attitude change; they are also significantly related to each other. Susceptibility to change was found to be inversely proportional

to the intensity of the original position. As the author modestly implies, these are not new and unexpected findings. The merit lies in the fact that the study goes beyond demonstration that these factors make a difference toward more precise statements of the nature of these functional relationships.

An investigation with an interest in the possibility of consistent individual differences in response to social stimulation is Janis & Field's (58) study of responsiveness to persuasion. Earlier studies by Janis (55, 56) had demonstrated that, over a relatively small range of situations, there were consistent individual differences in persuasibility. To study these problems further, Janis & Field have developed a test of persuasibility, which measures the opinion change produced by ten persuasive communications that vary widely in topic and type of appeal. The test has substantial reliability. Results with 185 high school students indicate that there is a general persuasive factor; persons most readily influenced by one communication are also most likely to be influenced by others in different areas and also by communications which are completely opposed to the first one. In general, actual opinion change scores did not relate significantly to self-ratings of susceptibility.

Prejudice and stereotyping.—In the eighth of a series of interesting papers on personality ratings and pictures, Secord, Bevan & Katz (95) report some findings that extend our understanding of the functioning of stereotypes. Subjects whose attitudes toward Negroes were known rated 15 pictures, 10 of Negro faces and 5 of white faces, on a series of traits. Ten were physiognomic traits known to be characteristic of the Negro and 15 were personality traits generally regarded as part of the stereotype of the Negro. The Negro faces were previously chosen to present a considerable range of "Negroidness." The results indicate that judges with varying degrees of prejudice behave similarly in that all respond categorically; they assign traits to an individual on the basis of his group membership although the individuals are not similar in the extent to which they possess group membership characteristics. As the authors point out, this is a generally accepted but seldom tested part of the definition of a stereotype.

A closely related property of social stereotyping is the tendency to exaggerate differences between groups which are known to exist. This tendency Campbell (25) refers to as *enhancement of contrast*. The difference is in agreement with existing facts but it is exaggerated. Most attempts to account for stereotyping in general, and this aspect as well, involve primarily propositions about the basic personality structure and the social situation. Campbell proposes, however, that enhancement of contrast may be, in part at least, an evidence of cognitive principles which would produce it even if other factors did not. He suggests that the phenomenon of stimulus generalization would lead to the prediction that there would be stimulus confusion and the enhancement of contrast when learning is imperfect. Since no experiment had been done which resembles the situation, Campbell set about testing the adequacy of his derivation. Two similar groups of nonsense

syllables were associated with a partially overlapping series of locations on a spatial continuum. As predicted, when learning was incomplete the differences between the two groups of nonsense syllables were exaggerated. In an interesting discussion of this experiment Campbell observes that social psychology tends to use the terms *perception* and *judgment* to describe the products of experience. Meanwhile general psychology and learning theory in particular are coming more and more to use concepts such as *habits* and *response tendency* to describe these products. This difference in terminology had led to undesirable isolation of these areas from each other. Campbell suggests that these language systems are often interchangeable. Thus one can speak of perception of people, judgments of people, and responses to people with little alteration of meaning and no differences in prediction.

Authoritarianism.—The effort continues virtually unabated to find correlates of the F-scale scores and thereby increase our understanding of the measure, to clarify our conception of the authoritarian personality, to test hypotheses formulated by the originators of the F scale, or to reach all three of these objectives together.

One of the consistent propositions about the authoritarian personality has been that it is particularly characterized by feelings of hostility. This proposition was subjected to careful test by Siegel (100) who related F scale scores to two measures of hostility. The first of these consisted mainly of items drawn from the Minnesota Multiphasic Personality Inventory (M.M.P.I.) comprising a manifest hostility scale and the second was the Elizier Rorschach Content Test, a projective measure of hostility. There was a significant tendency for high F subjects to show more hostility on the manifest hostility test. A less vigorous relationship in the opposite direction was obtained with the projective measure of hostility.

Kogan (68) reports an investigation designed to test the hypothesis that "recognition accuracy for stimuli consisting of disparaging aggressive and sexual references to parents and self is inversely related to the strength of authoritarian and ethnocentric attitudes." This hypothesis is based upon the proposition that there is a strong association between authoritarianism and repression. Verbal statements of aggressive, sexual, or neutral content were presented to male college freshmen under noise-masked conditions. The results were that the higher the F score the poorer the recognition accuracy. The hypothesis concerning ethnocentricity was not confirmed, however; the E-scale scores were not related to perceptual performance.

Another item in the search for the correlates of authoritarianism is a study of the intercorrelations among measures of concern for status, authoritarianism, and anti-Semitism. Kaufman's (64) scale to measure concern with status contains items such as: "Possession of proper social etiquette is usually the mark of a desirable person" and "In order to merit the respect of others, a person should show the desire to better himself." This ten-item scale correlates .71 with a fifteen-item scale chosen from the items of the F scale and .66 with the A-S scale. When the concern for status score is partialled out

there is no significant relationship between the F and the A-S scales. Conversely, the relationship between status concern and anti-Semitism remains when the F-scale score is partialled out. These findings are consistent with considerable evidence that prejudice is correlated with social mobility and social marginality (15, p. 55).

As the studies cited above indicate, there is a growing body of data demonstrating the usefulness of the conception of authoritarianism. There appears to be a good deal of stability in the correlates of authoritarianism and growing support for the original conception, although it is being modified and refined. The evidence grows showing that the manifestations of the authoritarian personality are widespread. An important part of the basic conception receives a severe jolt, however, in the work of Burwen & Campbell (24). Basic to some of the propositions in the conceptualization of the authoritarian personality is the assumption that such a person has developed a particular attitude toward authority figures. For that matter the notion is a common one that attitudes toward authority have their origin in relationships to parents and these attitudes are generalized to other authorities in later life. Burwen & Campbell used a wide variety of measurement techniques including interviews, the Thematic Apperception Test, self-trait check lists, photo-judging tests, autobiographical inventories, attitude surveys, and sociometric measures to determine whether there was an entity that could be described as a general attitude toward authority. Twenty-one measures of attitudes toward father, peers, symbolic authorities, boss, and symbolic peers were obtained. The intercorrelations provide no evidence whatsoever that a generalized attitude toward authority figures exists.

Since the experimenters used many different and standard measuring devices, the general collapse of such a widely proposed hypothesis is something to ponder. Perhaps one is about to witness a rare event in social psychology indeed: the rejection of a prominent hypothesis. No such thing. The authors suggest that probably the theory should be modified in terms of the conditions under which it holds. The original proposition is probably sound for psychotics and extreme neurotics and perhaps in imaginative thought processes like dreams. The normal waking individual does not, however, have a general attitude toward authority figures; the boss and the father and the symbol of authority are not similar for him. In spite of this caution, it seems reasonable to argue that in the form in which it is usually stated a formerly respectable hypothesis deserves to be buried. However, as the authors themselves suggest, it is unlikely that such will be its fate. It is certainly unlikely that the notion of a generalized attitude toward authority will drop out of the conception of the authoritarian personality.

A study by Paul (82) represents an intriguing amalgamation of insights from several areas of investigation. He brings together results from studies of first impressions, the authoritarian personality, attitudes toward power figures, changes in personal attractiveness as a result of external events and the effect of needs and dispositions on impressions of personality, and the

effect of accomplished facts on attitudes and opinions. Paul integrates these insights and hunches to make predictions about the relationships among impressions of personality, authoritarianism and the *fait accompli* effect. Taking advantage of the presidential election of 1956, which provides a *fait accompli par excellence*, Paul predicts that the election outcome will produce changes in the impressions people report of the personalities of the candidates and that, in general, the winning candidate becomes more attractive. The second prediction is that the authoritarian person is more susceptible to the *fait accompli* effect than are equalitarian people, i.e., he particularly will come to admire the winning candidate. Both of these predictions are borne out.

COGNITIVE PROCESSES

Some of the most exciting items in this year's literature are those in which an experienced and productive researcher appears to have sat back, collected his thoughts, and started on a new and stimulating attack on an old problem. The discussion by Rokeach (90) on the essential unity of thought and belief is an instance. He observes that there have been many experimental demonstrations, notably springing from the provocative conception of the authoritarian personality, of the relationship between social beliefs and aspects of cognition. Thus ethnic prejudice has been found to be related to measures of rigidity (11), concreteness (89), and premature closure of perception (18). By and large, efforts to account for these results have emphasized conceptual systems derived from personality theory. Rokeach, while not denying the fruitfulness of the personality approach, proposes that belief and cognition are similar processes and are governed by the same laws. He thus proposes that the relationships heretofore discovered may be illuminated by a study of the organization of cognitive processes. As Rokeach himself observes, the possibility that there is a structural identity between thought and belief is not a new one. The merit of the presentation is that it goes from this conception to research. The article presents a conceptual model which ties together the organization of belief with the organization of thought. The research described is derived from this model and the results indicate that the way in which a person solves problems is very similar to properties of his belief system. This analysis of the structural properties of belief and thought, independent of the substantive content of these, may provide more precise direction to the search for determinants of belief systems. It is very possible that the search up to this time has been too much influenced by the actual content of the beliefs.

Modern social psychologists are particularly poor historians when it comes to searching the literature. The usual study cites two or three intellectual predecessors and these of recent date. If one were to judge from contemporary articles, experimental quantitative investigation of social processes dates from the late 30's, probably with the autocratic-democratic leadership studies. There are undoubted losses in time, effort, and knowledge as a result of this neglect of history. To document this point one might cite

McCurdy's (79) gentle reference to studies in 1902, 1903, and 1906 bearing on the effect of value on perception, none of which has earned a reference in the contemporary studies of this problem. McCurdy also cites a study in 1913 in which subjects were asked to indicate which of 6 circles corresponds in size to a penny, nickel, dime, quarter, half dollar, or dollar. The results reported resemble closely those reported by Carter & Schooler (26). McCurdy's purpose in the citation was not, however, to illustrate the ahistorical bent of modern social science. He observes that in all the coin studies there is a correct ordering of the size of coins and a slight exaggeration of the difference between them. He suggests that the exaggeration of differences helps to preserve this order, which is itself important to the individual. McCurdy suggests that the concept of schema "as an orienting and stabilizing factor in new contact with the environment . . ." (p. 164) is a useful one to explain the results thus far obtained. A schema, such as the memory schema for coin size, is hypothesized to resist sensory data; the perception of objects included in such schema is likely to be less influenced by actual sensory contact with the objects than items not involved in a schema. Although evidence for the effects of needs and values is fragmentary, the possibilities for such effects exist, and McCurdy suggests that they are probably highly individual and are obscured by group averages. Needs and values, he speculates, have their effect on schemata rather than upon the perception of the object directly. The approach suggested here resembles Campbell's (25) discussion of enhancement of contrast, mentioned above.

INFLUENCE AND OTHER INTERACTION PROCESSES

In an experiment with the delightful flavor of both Asch and Piaget, Luchins & Luchins (75) studied the process of influence as it operates when two people differ in their interpretation of stimuli. In these experiments two children, between 10 and 12, are given the task of measuring some object. Unknown to them, one is given a metric and the other a linear ruler. In each instance the measurement was made and announced first by the child using the linear ruler. On over 700 measurements made by 60 pairs of subjects in three experiments, there was no conformity to the overheard response in more than 96 per cent of the instances. About 40 per cent of the pairs discovered the source of their disagreement without any hints from the experimenter. Only one pair of the 60 failed to discover the difference in rulers even with the hint. As the measuring task increased in complexity, that is, as the number of objects to be measured increased, the subjects became less certain of their measurements, became less inclined to suspect the rulers, and took longer to discover the source of disagreement. There are some provocative results showing that the children with the linear ruler (who measured first) were more confident of their measurements, more vocal, more inclined to suspect the rulers, and more likely to discover that different units were being used. The experimenters observed that the subjects regarded the disagree-

ment as a problem which they could solve. They speculate as to what would have occurred if the subjects had been more ego-involved about their responses and their frames of reference, if the frames of reference were related to prized personal or social values, and so on. This simple design lends itself excellently to the study of these important problems.

Occasionally one is especially struck by the fact that much of the activity in social psychology, and probably social science generally, consists in setting the stage for determining the magnitude of a particular phenomenon. A case in point, although many might be cited, is a study by Mellinger (80). His hypothesis was that "a communicator B who lacks trust in the recipient of his communication, A, tends to be motivated to conceal his own attitudes about an issue, X, in communicating with A. The accuracy of A's perceptions (of B's attitudes) is impaired accordingly." This hypothesis has undoubtedly been in the folklore for centuries; it has a great deal of anecdotal support. Not surprisingly the hypothesis is confirmed. The study merits attention on two grounds: it tests the hypothesis in a large organization and it sets the stage for a careful study of the amount of distortion which takes place as a result of distrust under various conditions. Knowledge advances from the question: Does it make a difference? to How much difference does it make?

Some of the most famous names in social theory, Mead, Dewey, and Cooley, have discussed the process of symbolic role taking or empathy. The amount of empirical investigation of the process has been relatively limited, however, perhaps because of the reasonableness of the speculations. There is a naturalness and acceptability to the idea that every social act involves putting one's self in the role of the other imaginatively, so that little empirical demonstration appears necessary. Methodological and conceptual difficulties may also be responsible for some of the neglect (50). In spite of these problems, progress is being made in the study of this process. Using the technique suggested by Dymond (40), Vernon & Stewart (111) studied empathy in the dating situation. Respondents were asked to indicate their guesses as to the satisfaction of their dating partners in fourteen areas. The index of empathy was the discrepancy of these judgments from the satisfaction ratings actually given by the partners. The results indicated that greatest empathy was present when the relationship was other than casual; it was highest among those who were engaged or going steady. Empathy went up as the number of dates with each other went up. There was no relationship between empathy score and sheer amount of dating. The authors interpret this to mean that empathy is not exclusively a matter of personality. The capacity to empathize develops over time and is influenced by situational factors.

Another study of the accuracy of social perception is that of Baker & Sarbin (8). The study began as an investigation of the possibility that delinquents would differ from nondelinquents in the accuracy of their social perceptions. The hunch that delinquents would be less accurate was not

confirmed. Analysis of the data indicated, however, that the two groups differed in the way in which they achieved accuracy. The study is cited here primarily for the careful thought the authors have given to the problem of measuring accuracy of social perception. They suggest that prediction of the behavior of others is an achievement variable, probably influenced by many factors. As such it cannot be accounted for by a single mediating variable, such as empathy, for example, and cannot be adequately assessed in a single experimental situation. They suggest that the Hasdorf & Bender (50) correction of the accuracy score for the influence of projection is a gratuitous correction for but one of the many factors which may be operating. They argue that the situation calls for an adequate measure of accuracy of social perception and, following this, a systematic study of the mediational processes which influenced accuracy of social perception.

Concern with empathy and accuracy of social perception indicates the appropriate interest of the social psychologist with the impact of the social climate on individual behavior. The attitude toward a person held by his peers has long been an important part of that climate. Croft & Grygier (37) point out that in spite of this central interest there has been little investigation of the relationship between sociometric status and adjustment. One investigator has reported a significant positive correlation between sociometric status and social adjustment as measured by the California Test of Personality (19). Croft & Grygier refer to a British study which indicated that a simple sociometric questionnaire distinguished more satisfactorily between neurotic and normal children than did 29 other measures derived from personality inventories.

Croft & Grygier's own study concerns the social relationship of truants and delinquents. This study is crammed full of interesting material which contributes not only to the understanding of the relationship of sociometric status to delinquency but also to the preference/rejection process in social groups. They found, for example, that rejection scores were more reliable than preference scores, that popularity was spread evenly over the group while rejection tended to focus on a few, that increasing the size of the groups served to increase the number of the rejected youngster's enemies leaving his number of friends unchanged, and that the rejection score was a better predictor of behavior as rated by teachers than the preference score. In general, the rejection score appears to be more important than the preference score. The authors make the interesting suggestion that the sociometric procedure can be used to provide information concerning the extent to which the value of the institution and of the treatment staff is accepted by the inmate community. If the most popular boys as far as their peers are concerned are those designated by the treatment staff as most troublesome and disturbed, it is at least clear that the boys and the staff do not share the same values. To return to one of the main ideas of the study: in general, delinquents and truants had lower sociometric status than other boys and the authors suggest that delinquency may in part be a defense against this social isolation.

GROUP DYNAMICS

The long standing interest of the social psychologist in the impact of the group on individual attitudes and behavior continues. By and large, this whole area of research reflects a steady increase in sophistication. Early studies were content with demonstrating that the group does or does not have an influence. Modern studies are concerned with specifying the conditions under which effects are demonstrable and the differences between conditions in terms of the magnitude of their impact on individuals. The next step, and instances of these are fewer, occurs when the experimenter specifies not only the conditions but the properties of the individuals which modify or enhance the impact of the group.

Conformity.—In this tradition of progressively increasing complication of theory is the series of studies on conformity to group norms. Back (7) has demonstrated that the tendency to conform increases with the attraction of the group to the individual. A study by Dittes & Kelly (38) holds this variable constant and varies the extent to which the person believes that he is valued by the other members of the group. Specifically, the prediction was made that: "Among persons who attach equal importance to their membership in a group, these who receive information that they are only minimally accepted by their colleagues and that this evaluation is subject to change, possibly becoming worse, conform more than persons who receive information that they are highly accepted and that this situation is stable." An earlier study (65) provided correlational support for this hypothesis but also demonstrated that it is difficult to manipulate acceptability artificially without simultaneously altering the attractiveness of the group. The discovery that one is barely accepted results in a lower attractiveness of the group. This decline in valuation reduces the desire to stay in the group, which desire underlies conforming behavior. In the present study this problem was partially obviated by motivating all subjects so that membership would be highly valued by all throughout the experiment. Conformity was measured in terms of agreement with group decisions on several issues after pressures to deviate have been applied. In those comparisons in which the valuation of the group was constant, the predicted relationship was obtained. The least conformity occurred in the condition under which the greatest acceptance was present. Two contrasting patterns of conformity were noted. In one there is a high degree of genuine acceptance of the norm which persisted even under private conditions. This pattern showed up in the condition under which the subjects believed themselves to be less than completely accepted but perhaps saw the possibility of becoming such. The second pattern was that of conformity only under public conditions and occurred in the experimental condition of lowest acceptance. These subjects differed significantly from the others in that they perceived rejection by the group as their probable fate. The experimenters suggest that these subjects perceived public conformity as a way of avoiding this eventuality.

The suggestion was made earlier that the next step in the development of

understanding of these situations comes from the introduction of personality measures into the design. The Dittes & Kelly study is a case in point. The high acceptance condition in that experiment is similar to one which is presumed to characterize in an enduring way the approach-oriented, high-affiliation person. Correspondingly the status of the individual in the low-acceptance group resembles that of the person whose dominant concern in affiliation situations is fear of rejection. It seems likely that categorization of individuals in terms of these tendencies would make more precise the predictions about the effects of these conditions.

The observation has been made earlier in this paper that science goes from the gross demonstration that A affects B to more precise specification of the conditions which influence this relationship. An excellent illustration of this process is provided by a study of conformity by Thibaut & Strickland (109). Several studies (43, 66) have demonstrated that the tendency to conform to group norms increases as attractiveness to the group increases. Thibaut & Strickland point out that there are instances in which conformity, although expected, does not occur. They propose that the set or orientation of the group member is a critical factor in determining which event will occur. They suggest that when the orientation of a group member

is to maintain or achieve membership in the group (group set), he will respond to increasing amounts of conformity pressure by increasing degrees of conforming behavior. When the group member's set is primarily cognitive or task oriented (task set), the perceptual or attitudinal reports of other members serve as confirmations or disconfirmations of his own judgments. Given that the group member is in some measure committed to an initial judgment and makes public the degree of confidence with which he holds his initial judgment, it is argued that increasing disconfirmations of his judgment (pressure to conform) will lead to decreasing conformity behavior

(p. 128). These hypotheses were confirmed in an experiment designed to test them.

Situational factors influence the extent to which a group norm influences conformity. The nature of the relationship of the individual to the group influences the degree of conformity. Personality traits have been found to relate to the tendency to conform to group opinions. Implicit in this latter conception is the notion that conforming behavior has a great deal of generality, that tendency to conform is a trait characterizing individuals in consistent ways, irrespective of the situation. Even if one were to conceive of the act of conformity as instrumental to the satisfaction of individual needs and hence to some extent situational, it might nevertheless become a general response high in the hierarchy for some people and less high for others. The extent to which conformity behavior is general across a number of situations and hence individually determined is under direct investigation in a study by Blake, Helson & Mouton (17). Using a simulated group technique in which the subjects by means of earphones hear a standard background, these experimenters found consistent individual differences in amount of conformity across three tasks: judging the number of clicks in a metronome,

expressing attitudes toward statements about war and peace, and solving arithmetic problems. Subjects who yielded to group pressure on one task showed a significant tendency to do so on another. The range of situations is small and the source of the norm (the background) is the same in all of them. Thus the evidence for generality provided by this experiment is restricted. Nevertheless, the study adds significantly to our knowledge and sets the stage for other systematic studies of this problem. To maximize the gain it is fervently to be hoped that, with the addition only of more kinds of tasks, the next experiment will replicate this study exactly.

Interrelations among process variables.—Borgatta & Bales (20) report an investigation of the relationship between sociometric position and interaction behavior as recorded by observers using the Bales category system. The analysis is based upon 125 men who participated in 166 three-men groups. The sociometric scores and the interaction scores for each person are based upon his scores in four different group sessions; as such, the authors suggest that they may be regarded as general tendencies of the individual. Four sociometric ratings were used: participant rating of leadership, own rating of leadership, popularity, and self rating of popularity (confidence). Analysis revealed that these four variables are significantly related to each other. Many of the sociometric types behave as one might expect they would from the verbal description of their sociometric position. Thus, for example the person who is designated by peers as a leader and recognizes himself as such but is unpopular and is mistaken about it has the highest rate of interaction. The authors suggest that the lack of popularity fits in with the picture of high activity but low sensitivity. Other sociometric patterns appear in other than chance frequencies and in most cases the behavior demonstrated makes similar sense. There is no claim here that the results are definitive. There is, however, the pleasant demonstration that differences in sociometric position have consistent behavioral consequences. Whether the classification of participants suggested by Borgatta & Bales is the most meaningful that might be developed depends on later research.

The Berkowitz (13) study of the relationship between being chosen leader on the one hand and social desirability and frequency of influence attempts on the other also illustrates the value of combining sociometric measures with those of actual behavior in a single study. The subjects were officer candidates working in groups of six on the task of constructing a foot bridge. Peer ratings of leadership and social acceptability were obtained immediately after the task was finished. Eight weeks later and again eight weeks after that (near the end of the course), peer ratings of effectiveness were obtained. The actual behavior of each participant in the work situation was categorized into leading and non-leading categories. The immediate leadership choices correlated significantly with both the peer ratings of effectiveness obtained later. The other indices of behavior and the measure of social acceptability were not significantly related to these later peer effectiveness ratings. The intercorrelations of scores obtained in the work

situation confirmed the expectation that men who were desired as leaders made more influence attempts and were also more acceptable social companions. These people were preferred over less acceptable companions who made many attempts to direct, and over people who were acceptable but who infrequently attempted to direct others. These results dovetail nicely with those of Borgatta & Bales.

The evidence is substantial that accurate social perception is a positive property and is associated with position of leadership (32). The relationship between accuracy of social perception of members of a group and the group's performance is not so clear; sometimes it has been a beneficial effect and sometimes there has been no evidence that it makes a difference. Steiner (104) has helped to clear up the confusion by specifying the conditions under which accurate social perception might be expected to have an effect on individual and group efficiency. One of these is that the individuals who have accurate perceptions must be free to adjust their behavior in accordance with their perceptions and a second is that the behavioral effects of the perceptions must be such that they produce more integrated and coordinated group behavior. Steiner & Dodge (105) propose that role systems prevent the occurrence of these conditions and obscure any relationship between accuracy of social perception and individual or group efficiency. To test these ideas they set up four conditions, manipulating the accuracy of perception and the presence or absence of a set of prescribed roles, and compared the performance of groups under these conditions. They found accuracy of perception significantly and positively related to performance only when there was no role system. In the absence of a role system, accuracy was also associated with indices of high cohesiveness.

Group structure.—The impact of practical demands that social psychologists join engineers and others in the study and design of complex man-machine systems shows itself in the current literature. In general the resulting investigations have concerned themselves with the effect of various types of group structure upon measures of group performance. Two such studies are reported by Lanzetta & Roby (70, 71) who are particularly interested in the impact on performance of communication variables such as the volume of information to be relayed, the extent to which relevant information is dispersed throughout the group, and the nature of the communication pattern. The group task used in these studies is modeled after an air defense situation. In both studies there is a clear performance decrement as the work load increases. A close analysis indicates that this is not primarily a matter of the amount of information being relayed but rather of the way in which the information is processed and transmitted. The authors suggest that the limiting factor for the performance of groups may lie in the ability of the group to set up efficient systems for transmitting information rather than gross information capacity. These authors have also developed a conceptual model for the study of task performance and group structure (88).

Another study of the relationship between the nature of the communication process and group performances is that of Shaw (98). He observes that in most of the studies of this problem in the past the necessary information is spread randomly throughout the group. He reports a comparison of this condition with one in which the information is systematically distributed among the participants and the distribution system is known. In general, the results showed that the systematic information distribution was superior to the random one in terms of speed of problem solutions, number of errors, and participant satisfaction. Knowledge of the nature of the distribution spread was not a significant factor largely because the knowledge was not used. Shaw suggests that the organized distribution of information imposed a useful kind of organization on the content of messages transmitted. The systematic distribution grouped information items in a manner useful for problem solving.

LEADERSHIP

Smith (103) pointed out in the 1952 review of the literature that leadership no longer seems a simple phenomenon. The simple notion that leadership was a matter of a complex of traits was being altered to accommodate repeated evidence that the situation makes a difference. Two aspects of the situation which are currently receiving much attention are the personalities of followers and the tasks in which the group is engaged. A statement concerning the behavior of leaders which would probably receive general acceptance goes something like this: the behavior of leaders in groups is influenced by the personality of leaders, the requirements of the group task, and the needs, values, and expectations of the other members of the group. An important and complicating factor is, of course, the patterns of interaction among the members of the group.

In harmony with this general scheme are the studies by Haythorn *et al.* (51, 52) on the behavior of authoritarian and equalitarian personalities in small groups. While conceptualized as studies of leadership, they also have bearing on the general problem of correlating social behavior indices with personality measures. In the first report of this work (51) these investigators disclosed behavioral differences between groups which were homogeneous in that all members had high F-scale scores and groups which were homogeneously low on the F scale. The emergent leaders in the two groups were markedly different as well. In the low F groups, the emergent leaders were more sensitive to others, were more effective, and were less likely to give direct orders than were emergent leaders in high F groups.

The second report (52) deals with an experimental manipulation of leadership. In half of the four-man groups leaders were appointed who had F scores similar to those of the other members. In the other groups, the leader's F-scale score was at the opposite end of the distribution. The participants were rated by observers on 16 behavioral characteristics and the interaction behavior was categorized into a 43-category observation system. The results,

by and large, fit in with the previous findings with respect to high F and low F leaders. High F and low F followers also behave in consistently different ways. The high F followers, for example, were more satisfied with their leaders and rated their groups as more productive. In general they were rated as less equalitarian and as less sensitive to others. Other findings were that leaders with the same F-scale scores behaved differently depending on the characteristics of their followers. Conversely, the behavior of followers was found to be a function of the characteristics of the leaders. Differences between high and low F leaders depended upon whether or not the followers were high F or low F and vice versa. These findings represent substantial evidence for the importance of interaction effects in the study of leadership. By taking these interdependencies into account, the predictability of behavior in small groups is increased. The authors warn that the particular findings are probably influenced by the nature of the task given to the group.

In the folklore, if not in the scientific literature, there have been two quite different conceptions with respect to similarities and differences between leader and followers. One view has been that people are more happy with leaders who resemble them in important respects. The other is that people want leaders who are not like themselves. There are data in the Haythorn *et al.* study relevant to these ideas. Followers whose leaders were different from themselves were significantly more satisfied with their leaders than were followers whose leaders resembled them. It would be unwise to conclude that the issue has now been settled between the two points of view. It is rather the case that once again we are moving toward more precise specification of the conditions under which certain relationships are obtained. The nature of the task might dictate, for example, that in order for the group to operate successfully the leader should find activities congenial which are not pleasant or possible for followers. Under these conditions one would expect followers to prefer leaders who were different from themselves. It is easy to imagine other situations in which the opposite was true.

The distinction between popularity and leadership has been accepted for a long time (59). The empirical relationship between them is not equally clear; sometimes the leader is also the most popular (48) and sometimes he is not. Theodorson (108) proposes that the cohesiveness of the group influences this relationship. When the group is cohesive member needs are being met, the leader is an important figure in that process, and he is, accordingly, highly popular. In groups of low cohesiveness, preferences for the individual are independent of role in the group. A study of two laboratory and two "natural groups" with varying degrees of cohesiveness tends to support these predictions. The correlations between ratings of popularity and leadership are generally higher in groups of high cohesiveness. These findings may help to account for the low correlation Slater (102) found between popularity and perceived leadership. This study is a nice instance of a common state of affairs in social psychological research: the support of the

hypothesis without direct support of the theory leading to the hypothesis. The fact that popularity and perceived leadership go together in cohesive groups may not be due to a necessary integration of these roles in cohesive groups as Theodorson reasons. It may be that cohesiveness influences the amount of generalization which occurs; perhaps the intercorrelations of all trait-ratings are higher in cohesive groups.

There are many reasons for calling attention to a recent study of leadership stability and social change by Katz and associates (63). For one, its introduction provides a nice treatment of the state of affairs now that the leaders are not supposed to have traits which differentiate them consistently from followers. The emphasis on the importance of the situation has been a wholesome one. Yet it does happen that sometimes leaders persist in leading after the group task changes and at other times they do not. This leads Katz *et al.* to consider the conditions under which leadership might be expected to be stable and to suggest that leaders are more likely to retain their leadership when the group has a voice in deciding its activities than when this is not the case. They report their efforts to test this hunch. A second reason for the reference is the nice illustration it provides of confirmation of the prediction even though several of the assumptions on which the prediction was based turned out to be unsound. Finally, the study illustrates the importance of variables not experimentally introduced but discovered in the process of the study. The developing processes of the group interacted with the experimental manipulations. The study is an excellent example of a careful reformulation of an important problem, of the complexities of small group theory and method, the insistence of unplanned sources of interference, and, above all, of the way in which careful and knowledgeable researchers can rise above these limitations and add substantially to our knowledge.

Along with the experimental investigations must go careful field studies of leadership to provide the descriptive information so badly needed. Quite a number of these are being done by political scientists interested in political behavior. One of these is a study by Belknap & Smuckler (12) of community leaders in a midwestern city of 50,000 by means of interviews in which respondents were asked to identify "the most important people in town when it comes to making decisions about local public affairs here in community A" (p. 75). There was a remarkable amount of coalescence of opinion; only 60 persons were named and of these 17 accounted for two-thirds of the nominations. There are some differences in the nominations of those who are active in public affairs and those who are not; inactive people are more likely to cite the formal leaders. The resulting analysis indicated that the community contained a relatively small group of top leaders whose influence was general. There is a larger group of secondary leaders whose participation and influence depend on the nature of the problem. Most of the leaders revealed by the interviewers were not formal, elected leaders; by and large

designation as leaders appears to be much more a matter of position than of personal characteristics. This pyramidal picture of leadership may be a general one. A very similar picture is suggested by Agger & Ostrom (3). These and other articles are contained in an excellent collection of papers on political communication edited by Pool & Bonilla (85).

A study thoroughly embedded in the interaction conception of leadership is that of Lowe & McCormick (74) who were interested in comparing the characteristics of informal and formal political leaders and the characteristics of the respondents who were influenced by each type of leader. The data were collected in the senatorial campaign in 1950 in Madison, Wisconsin. This technique of identifying leaders by nomination from respondents has been getting considerable use in studies of influence, political influence particularly. Several findings of this study are significant in the light of this emphasis. One is that there was a good deal of independence of voter opinion from the opinions they attribute to leaders, both formal and informal. A second is that informal leaders apparently have as much influence as formal leaders. Finally, the amount of influence of leaders of both types decreases as voter participation and interest increase.

METHODOLOGICAL CONTRIBUTIONS

The distinction between content and method is an unclear one in a comparatively young area of scientific activity. Methods rest insecurely upon inadequate theory and research results often contribute equally to a refinement of the measure and the development of theory. With this acknowledgment of the dubiousness of the distinction, this section deals with studies whose primary impact is methodological.

The student of techniques of analysis of survey data may study with profit Borgatta & Hulquist's (21) reanalysis of data from Stouffer's *Communism; Conformity and Civil Liberties* (106). They point out that Stouffer's presentation was based upon the familiar cross tabulation approach and, with such procedures, the importance of variables is indicated by their relative contributions to given phenomena. The questions posed in this article are: "How do these findings look when viewed from a correlational approach and what happens in factor analysis?" (p. 632). The results of the factor analysis provide corrections to conclusions arrived at from the first order tabulations and indicate that factor analysis provides "a provisional ordering of the data. . . ." The authors suggest that factor analysis might help to identify stable variables useful for the description of social situations. They also note some limitations of the factor analytic procedure, among them that subtleties and curvilinear relationships may go undetected.

The effort to provide sound theory for dealing with such problems as interview bias, coder bias, unreliability bias due to the form and phrasing of questioning, and other methodological issues goes on with increasing vigor and sophistication. Many references could be cited to document this asser-

tion, among them Dodd's (39) suggestion that the correlation between response to questions and behavior can be improved by considering the mood, tense, number, and voice of responses to poll questions; Franzen & Williams' (44) method for measuring error due to variance among observers; and Adams' (1) experimental study of the supposed general tendency of respondents to avoid strongly unfavorable responses. (It isn't general.)

For four years members of the staff of the National Opinion Research Center have been using tape recorders as part of the data collection apparatus in field settings. Bucher, Fritz & Quarantelli (22) report some systematic observations based on this experience. During investigations on the Disaster Project approximately 700 out of 1000 interviews were tape recorded. On the average the interviews yield 29 pages of typescript. The respondents, because disasters are indiscriminate in choosing victims, cover a wide range of social and personal characteristics. The authors have written a precise and sage, brief handbook for the use of tape recorders in field settings. The value of this kind of contribution to the literature is hard to overestimate. Wise and careful statements of experience can save fellow investigators much time and trouble.

A similarly worthwhile collection of essays on the interview, written by experienced and observant investigators, is contained in the September, 1956, issue of the *American Journal of Sociology* edited by Reisman & Benney (87). For the most part these essays draw primarily on the experience of the authors and deal with such topics as the effect of age and sex of the interviewer and the respondent, the use of role playing in the interview, respondent attitudes toward interviewing, biased questions, and depth interviewing. A series of careful, thoughtful studies on the clinical interview by Kounin *et al.* (69) and by Polansky & Kounin (84) also constitutes an important contribution to the literature on the interview. Although the perspective is quite different from the essays concerning the survey interview, these experimental studies provide important insights into the basic two-person interaction situation which the interview is. Another important contribution to understanding the interview is the book by Kahn & Cannell (62). Based upon extensive experience with the survey interview, the book deals with such topics as the interview as an instance of social communication, the psychological basis of the interview, the formulation of objectives, and the interview as a method of measurement. The second part of the book contains several illustrations of interviews directed at the objective of obtaining information.

Imagery measures.—In view of the extensive use now being made of Thematic Apperception Test-type assessment techniques, Scott's (94) methodological study has important implications. The general assumption behind this procedure has been that the more content related to a particular theme the stronger the underlying process or tendency. Scott's analyses indicate that this is not necessarily the case. In three studies Scott found a

substantial number of subjects who decreased the amount of imagery related to a threatening event under a high cue as opposed to a low cue situation. The stories of these subjects, labelled avoiders by Scott, showed more instances of fear. These results together with those of Clark (34) indicate the importance of the situation in which the measurement takes place and of the content of pictures. It is clear that an ordering of people in terms of the strength of an approach tendency can be quite different if the situation and the pictures are altered in specified ways. If the event or the cues in the situation or in the picture give rise to fear, one of the consequences can be a decrease in the amount of writing or talking about the event. It is clear that persons who differ in the extent to which they fear an event will differ in the way they react to an increase in cue strength.

A study reported by Kagan (61) is relevant to this general problem of the conditions under which ordering of subjects based upon fantasy material approximates the ordering obtained under other conditions. The relationship between overt aggression and aggression in fantasy has been studied many times with contradictory results. Some investigators have found a significant positive relationship between these two while others have found no relationship. Kagan's hunch was that there would be a significant relationship between fighting behavior and aggression shown and stories told to pictures if the pictures were not ambiguous but actually exhibited aggressive content and if the fantasy cues were similar to the overt behavior in expression and goal object. Both predictions were confirmed. Children rated as likely to fight produced significantly more fighting themes. Further, differences between aggressive and nonaggressive boys did not show up on the ambiguous pictures. Kagan also reports data which remind one of the problem with which Scott deals: failure to provide aggressive themes to pictures that strongly suggest aggressive content may indicate anxiety over the aggressive thoughts and behavior. Finally, the article by Leary (72), presenting a theoretical discussion of fantasy productions and a method for measuring interpersonal aspects of imaginative expressions, is certainly of interest to the many students of TAT-type measures.

Scales.—The methodological skills and conceptual systems of the social psychologists have come into demand in the management of mental hospitals as a consequence of an increased awareness that the treatment process in an institution is not limited to formal treatment relationships. For better or for worse all aspects of the entire institution are involved in or have an impact upon the effectiveness of the therapy program. An important part of the total climate in a mental hospital, especially in the light of this growing conception of the hospital as a community all members of which are involved in treatment, is the ideology of the members of the community regarding the nature and cause of mental illness.

Gilbert & Levinson (49) report the construction of an ideology scale which reflects an individual's preference for either the old or the new view-

points concerning mental illness. Additional objectives were to investigate the personality contexts correlated with these orientations, the ways in which the hospital's overall policy relates to the most common ideology, and the modal personality of the staff members. The traditional pattern of treatment is labelled by the authors as "custodialism"; the newer orientation, "humanistic." "The model of the custodial orientation is the traditional prison and the 'chronic' mental hospital which provide a highly controlled setting concerned mainly with the detention and safekeeping of its inmates. Patients are conceived of in stereotyped terms as categorically different from 'normal' people, as totally irrational, insensitive to others, unpredictable and dangerous" (p. 264). In contrast, the humanistic orientations emphasize therapy rather than custody. Patients are viewed psychologically rather than morally. The expectations concerning the effectiveness of treatment are more optimistic. A Custodial Mental Illness (CMI) Ideology Scale was constructed to measure the tendency toward one or the other of these orientations. Although admittedly crude, the scale is reliable, internally consistent, and reflects expected differences between validation groups. The authors, proceeding on the assumption that these ideological positions are part of the personality structure, predicted that the CMI scale scores would be significantly related to F-scale scores and scores in the Family Ideology scale. This prediction was confirmed. The custodial orientation in a mental hospital setting is, apparently, but another manifestation of the authoritarian personality structure. Ideology is not exclusively a matter of personality structure, however; it is influenced by the social situation. Evidence for this is provided by the fact that there was marked congruence between prevailing policy of the hospital, the modal ideology, and the modal personality. "The hospital units with the most custodial policy requirements had as well the most custodial modal ideologies and the most authoritarian personalities." In addition to the contribution inherent in the development of a scale which probably has broad utility this study is an excellent example of an individual and a collective investigation. The same phenomenon, ideology, is studied with the individual and with the organization as the unit of analysis.

Dogmatism, rigidity, authoritarianism, and ethnocentrism are concepts in frequent use in discussions of social behavior, and measures of them have been developed and widely used. The progressive refinement of the measures by means of correlational analysis is an important part of the current activity of social psychologists. Rokeach & Fruchter (91) report a factorial study designed to see whether "the concept of dogmatism and its operational measurement by means of the Dogmatism scale has content which distinguishes it from other well-known concepts" such as those mentioned above. The following battery of ten scales was administered to 207 subjects attending college in metropolitan New York: Anxiety, Paranoia, Self Rejection, Dogmatism, Authoritarianism (F scale), Rigidity, Ethnocentrism

(E scale), Political-Economic Conservatism, Left Opinionation, and Right Opinionation. On the basis of the factor analysis it would appear that the Dogmatism scale does measure something different than do the Rigidity, the F and the E scales. The latter two are oriented toward the right on the liberalism-conservatism dimension and are relatively unrelated to the anxiety factor. The Dogmatism scale is independent of the conservatism dimension and has a high loading on the anxiety factor. Dogmatism differs from rigidity in that the latter has its only loading on the rigidity-authoritarianism factor. The three factors found, with their tentative labels, were: Factor 1, Anxiety, composed primarily of anxiety, paranoia and self rejection, with dogmatism having a significant loading; Factor 2, Liberalism-Conservatism, composed of left and right opinionation, Political-Economic Conservatism scale; Factor 3, Rigidity-Authoritarianism, consisting primarily of rigidity, F scale, and dogmatism.

Measurement of influence.—March's (77) study of the problems in the measurement of influence is especially important in view of the great interest currently in the influence process. He presents data in terms of which eight different but common procedures for measuring influence can be compared. In general the correlations among the measures are positive but low; more than half are below .30. Each of the 50 subjects was ranked either most influential or least influential in his group by at least one measure. The interrelations among the measures were influenced by the nature of the task (high or low relevance) and by type of group (whether composed of friends or strangers). As March indicates, these findings mean that comparisons of research results from different studies using different measures of influence in groups which differ in composition and in task are hazardous.

Sociometry.—This reviewer must make apologies as did his immediate predecessor for the failure to give just due to the foreign literature. He would be even more derelict, however, if he failed to call attention to the excellent study of the sociometric method by Bjerstedt (16). In addition to a very useful analysis of the various meanings the term sociometry has acquired, the monograph discusses general principles for the collection and analysis of socio-preferential data. Bjerstedt points out that sociometric choice status scores are presumed to be "(a) relatively unaffected by preference method; (b) relatively unaffected by choice aspect (criterion); (c) relatively constant over a period of time; (d) accompanied by the individual's awareness of his own relative socio-preferential situation; (e) correlated with measures of general 'adjustment' and 'efficiency' of the single individual; and (f) possible to interpret similarly in different countries" (p. 8). The studies described in the monograph were designed to test these assumptions. In addition to a general study of 867 school children the monograph reports an intensive analysis of 136 subjects with extreme socio-preferential status scores.

Response set.—The observation has been made that the F and E scales

and many other attitude scales consist of statements all of which are scored in the same direction (9, 36). This has led to two somewhat contradictory suggestions: (a) that this scoring characteristic may spuriously inflate correlations between measures purporting to get at different aspects of the personality, and (b) that there is a general personality trait, a tendency to acquiesce, which is responsible for obtained correlations and that this trait is itself important. A logical outcome of this process is the scale for measuring social acquiescence developed by Bass (10). Acquiescence is defined as a tendency to accept any generalization about behavior and Bass suggests that it is a parsimonious explanation for a large part of the variance among personality measures. The scale consists of 56 items developed by internal consistency item analysis and has a reliability of .90. Based upon results obtained with a large number of quite different populations there appear to be consistent individual differences in the tendency to acquiesce.

Another study with the same intellectual history but which makes a different proposal is that of Chapman & Campbell (31). They report efforts to revise the F scale by reversing the wording of some F scale items. Jackson, Messick & Solley (54) report an attempt to determine whether the relationship which has been found between authoritarianism and rigidity might not be due to this proposed tendency to agree. Both authoritarianism (high F scores) and Einstellung rigidity, the authors propose, might be measures reflecting tendencies to acquiesce. Subjects were given the F scale, a reversed F scale, and a series of water bottle problems. Subjects who showed rigidity on the problems tended to agree with both the F and the reversed F scores, a result which to the authors suggests that both the Einstellung and the F scale reflect acquiescence and conformity tendencies.

Many of the empirical correlates of F-scale scores are predictable from the theory of the authoritarian personality but are not derivable from the conception of response set, or from tendency to acquiesce. It seems unlikely therefore that F-scale scores exclusively reflect either or both of these. A similar skepticism together with an interest in the methodological issues has prompted Christie and his colleagues to investigate the problem more thoroughly. I am grateful to them for an advance look at an article scheduled for publication in the *Journal of Abnormal and Social Psychology* (33). On the basis of several studies and the reanalysis of data from other investigations, they report some conclusions which clarify the situation considerably. These are: (a) that the F-scale reversals utilized in studies published to date are not psychologically meaningful reversals; (b) low scorers show little tendency toward acquiescence; most acquiescence occurs among those who show at most only a moderate tendency to accept the F-scale items, and, finally, (c) greatest acquiescence occurs with those who are in a situation of conflicting values. Greatest acquiescence appears to result from agreeing with two conflicting sets of values rather than a mechanical way of responding to test items or a blind tendency to agree with strong assertions. The indi-

viduals showing this behavior are, indeed, capable of entertaining incompatible propositions because of their own uncertainty. A definitive test of the impact of actual value conflict on the behavior now attributed to either response set or tendency to acquiesce should not be difficult to devise.

A NOTE ON THEORY CONSTRUCTION

There are many shades of opinion as to the optimum time to attempt to organize theoretical propositions into a formal theory. One position is that this must be done early because a logically precise system yields testable propositions which might not otherwise occur to the investigator. Another is to argue that a system developed on a small empirical basis tends to be too limited and is too much influenced by the assumptions of the mathematical system rather than by the nature of the data. Accordingly, for maximum usefulness the formal theory should have a broad base of empirical research. The nature of the process of compromising these extreme positions is illustrated in French's (47) presentation of a formal theory for social power. There has been a great deal of careful investigation of the process of social influence which permits the development of theorems consistent with empirical data. At the same time, however, French and his associates are obliged to make some guesses because the experimental base is not complete. The formal theory uses the theory of directed graphs, which does not require precise assumptions about empirical variables. Basically the theory reduces the process of influence in N person groups to a summation of interpersonal influences which takes into account three complex patterns of relations: (a) the power relations among members of the group, (b) the communication networks or patterns of interaction in the group, and (c) the relations among opinions within the group. Thus propositions which have been conceptualized at the group level (e.g., that the strength of group standards increases with increasing cohesiveness of the group) are deduced from concepts at the interpersonal level

(p. 181).

Another example of collaboration between mathematicians and social scientists in the development of a rigorous conceptual system is that of Cartwright & Harary (27). They present a formal theory, also using the mathematical theory of linear graphs, to deal with problems of structural balance, an issue which arises when one is concerned with systems consisting of interdependent parts. The effort is a more general extension of Heider's theoretical conception of cognitive fields containing perceived people and objects or events—a P (one person), O (another person), X (an object or event) system. The article illustrates some of the values accruing to the development of a formal system. The more precise formal treatment of Heider's theory of balance removes ambiguities in the theory, extends its applicability, and permits one to treat the concept of balance in terms of degree rather than in terms of presence or absence of balance. More importantly, the definition of balance suggested provides a rigorous method for

describing the relations among interdependent parts of empirical systems, and is applicable to a wider range of organizational settings than is the original Heider statement.

Simon (101) has assembled in a single place essays which he has written over a period of years in a consistent effort to use mathematics to illuminate social scientific problems. The essays deal provocatively with the substantive issues such as political power, bandwagon effects, interaction in small groups, pressure toward uniformity, and productivity. They can serve also as instructional material to those interested in the application of mathematics to social science.

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RECENT DEVELOPMENTS IN PSYCHOLOGY IN THE U.S.S.R.

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INTRODUCTION

In recent years, the history and contemporary status of psychology in the Soviet Union have been reviewed in the English language by Bauer (1), London (2, 3, 4), Razran (5, 6), and Simon (7), and a book of translations of Russian papers has just appeared (8). Nevertheless, the state of psychology in the Soviet Union seems to be only little known in the United States. It is advisable, then, to begin with a statement of general facts.

As Razran has recently pointed out (6), there were 19 Russian participants in the first international congress of psychology in 1889, and only three Americans. Russian psychology has had a considerable past. A psychological profession of a fairly large size is still in existence in the Soviet Union. Recent psychological meetings (in 1955-1956) are reported to have been attended by from 400 to 500 people. Psychology is a subject taught in secondary schools, universities, teachers' colleges, colleges of physical education, military academies, etc. According to Simon, psychological research is conducted in psychology departments of institutions of higher learning, in educational research institutes, in a number of psychological research institutes maintained by Academies of Sciences (or of Pedagogical Sciences) of the U.S.S.R. or of some of its constituent republics, in an Institute of Defectology, etc. Some of the research which might have been considered psychological in the United States is conducted by physiologists in the U.S.S.R. In the literature that was consulted, references were seen to Soviet editions of translations of some American books, e.g., Woodworth's *Experimental Psychology*, Watson's *Psychology from the Standpoint of a Behaviorist*, and Boring's *History of Experimental Psychology*.

Soviet psychology and Marxism.—The state of affairs in Soviet psychology has been markedly affected by the dominance of the Communist party in the country. Some aspects of the Marxist-Leninist ideology of the party could be and were utilized in attacks against the existence of psychology. Marxism-Leninism includes a philosophical system called dialectical materialism by its supporters. As such, it is opposed to all idealism and to "vulgar" or "mechanical" materialism. In terms of some interpretations of Marxism-Leninism, psychology defined as science of the mind is guilty of idealism; a behavioristic psychology is guilty of vulgar materialism. In terms of other interpretations of Marxism, psychology may be defended, and the defenders of psychology have the advantage that the "classics of Marxism-Leninism" had used psychological concepts freely and had treated psychology as the science of the mind. Nevertheless, the status of psychology appears to have been rather precarious. Ever since the 1920's discussions about the proper

tasks of psychology in the light of Marxism have been going on; at one time attempts to substitute a "reflexology" were prominent, and at present the point of view is sometimes presented that psychology should be absorbed by Pavlovian study of "higher nervous activity." Discussions of the relations between Marxist philosophy and psychology occupy much space in contemporary Soviet psychological literature and are parts of all the recent psychology textbooks which were examined [Teplov (9), Rudik (10), Ivanov (11), Egorov (12)]. There are many papers dealing specifically with the tasks of psychology in the light of Marxism, or with relations between Marxian epistemology and Pavlovian physiology, etc. A book and several papers dealing with the 19th century physiologist-psychologist Sechenov [vide Budilova (13)] are largely concerned with his contributions to a materialistic view of man. The issue of conformity with the Marxist teachings is also apt to enter into theoretical discussions of special topics. These discussions of the relations between psychology and Marxism will not be included in this review, partly because their history has been covered by Bauer and by London, and partly because the Russian attempts to establish the conformity of their points of view to Marxism impress this reviewer as largely unprofitable undertakings. Only a few examples will be given:

The following beliefs appear to be widely held by Russian psychologists in relation to the concepts of mind and consciousness. Psychology is generally defined as science of the mind (*psikhika*). The mind takes a new form in man, that of consciousness. Engels had stated that consciousness first develops in man as a result of participation in work. Does this mean that animals are assumed to be unconscious? Probably it doesn't, because minds are attributed to animals; mind includes cognition, feeling and will [Teplov (9)], and the Freudian unconscious mind is characterized as mythical. The picture seems to be confusing and probably cannot be clarified because of the need not to question the accepted Marxist dogma.

A lively controversy appears to be in progress about the concept of set. Two of the issues brought up in the controversy were: A set need not be represented in consciousness. If so, does it belong in psychology, which is the science of mind, or does it belong essentially to physiology? [Teplov (14)]. A set is viewed as determining perception, that is as an antecedent of the effects of stimuli on the organism. But in Marxism-Leninism the external reality is viewed as primary and its experience as secondary. Doesn't the doctrine of set treat sets as primary and stimulation as secondary? [Leontiev (15)].

The somewhat precarious position of Soviet psychology has probably contributed to the fact that for many years there had been no psychological periodical in the Soviet Union (from 1935 to 1954), and no psychological societies. At present, many of the Russian psychological publications tend to be reviews of research which has accumulated at research centers, rather than detailed research reports. In many instances it is not possible to reconstruct details of experimental procedures. This writer does not know whether

this is due to lack of available publication space, or whether there are other reasons for the omission of reports of research detail.

Russian psychologists have repeatedly referred to quotations from Pavlov's writings which indicate that he was not opposed to psychology. Thus recently a letter from Pavlov to the psychologist Chelpanov, congratulating him on the occasion of the establishing of his Psychological Institute, has been printed in facsimile (16).

Exclusion of certain topics.—A number of topics are excluded from Soviet psychology, apparently for political reasons. One such topic is intelligence testing; a resolution condemning it was passed by the Communist party in 1936 [Bauer (1)]. Social psychology is viewed as an un-Marxist subjectivistic approach to objective social phenomena. Psychoanalysis is commonly described as idealistic and as leading to political reaction.

Political influences and 1950 Pavlovianization of psychology.—Still another aspect of the political influences has to do with the effects of the Joint Session of the Academies of Sciences and of Medical Sciences in 1950, devoted to the teachings of Pavlov (17)¹. Razran (5) has pointed out that there were few references to Pavlov's work in textbooks written by Soviet psychologists before this meeting, and that there was a striking increase of quotations from Pavlov's writings in psychology textbooks after the meeting. Many references to Pavlov amount largely to lip service. Pavlov is apt to be quoted as saying that a scientist must be passionately devoted to truth; or, in a discussion of rote learning, it may be stated that rote learning is based on conditional responses, but without any attempt to relate the two groups of facts in detail. The proceedings of the meeting clearly indicate that the major decisions arrived at during the meeting represented political influence in science.

Utilization of foreign sources.—At the Joint Session a number of participants were accused of undue partiality to foreign science and of giving foreigners credit where Russians had the priority. The trend had begun earlier; Razran presents information which indicates that practically no sources other than the classical writings of Marxism had been directly quoted in the psychology textbook edited by Kornilov, Smirnov & Teplov in 1948. After the Joint Session Pavlov was added to the Marxist writers and the exclusion of foreign writers as sources of quotations continued. This information presented by Razran should not be misunderstood. The absence of quotations from foreign sources does not mean that these sources are necessarily unknown or not utilized. There are reasons to believe that they are utilized but without an indication of the source.

Doubts about complete sincerity of Soviet psychology.—In some instances,

¹ There exist two reports of this meeting, a short one which includes only the addresses of the two honorary presidents, and a full one including all speeches which were made and texts of the speeches which were submitted but not delivered. An English translation of the short version has been published.

this reviewer doubts the sincerity of certain passages by Soviet psychologists and believes that they only serve the purpose of demonstrating the political regularity and patriotism of the writers. Some quotations from the writings of the "classics of Marxism-Leninism," particularly from Stalin's writings, refer to points which have been made so many times by earlier writers, or which are so obvious in their content, that it is difficult to believe that the authors really viewed them as Stalin's contributions. More likely, they went out of their way to find quotations from Stalin in an effort to exhibit their party regularity.

There are factual claims by some Russian psychologists which may have a similar defensive function. Ivanov (11) claims in a discussion of individual differences that the greatest men of genius in the history of humanity were Marx, Engels, Lenin and Stalin, particularly the latter two. Many discussions describe the new Soviet man as if the hopes and wishes of the Communist Party had come true, and the Soviet Union were almost entirely populated by "typical new Soviet men," characterized by initiative, tendency to translate feelings into action, devotion to principles, modesty, honesty, etc. This very favorable view of the Soviet people is regularly presented, without any supporting evidence.² The lack of research on the "typical Soviet man" has been recently admitted by Kornilov (18). The favorable view of the Soviet people is regularly presented. Eres (19), in discussing undisciplined behavior of children, states that "as a rule the Soviet family is characterized by high moral qualities. . ." and "but there are separate (*oidelnye*) might be also translated as isolated, or as occasional) families which are not unobjectionable by far in respect to morals." Rudik (10) denies that Soviet athletes are interested in winning in individual competition for their own sake—they only want to advance sport in their country, are motivated by devotion to their teams, etc. However, the testimony of contemporary Russian novelists does not indicate that personal status motives have largely disappeared from the Soviet Union.

Foreign psychological literature is sometimes grossly misrepresented, and this reviewer doubts the sincerity of the pictures which are presented. Thus, American social psychology is apt to be characterized as generally "racist." Some of the misrepresentations are grotesque; thus Ananiev stated in a published popular lecture (22) that the absence of references to the soul in American psychology was a sign of prevalence of idealism; American psychologists did not discuss the soul so as to leave all the more opportunity for the clergy to do so.

Apparent change of status.—There are many indications that the position of psychology in the Soviet Union is changing. In 1955 a psychological periodical, the *Voprosy Psikhologii* (*Questions of Psychology*), began to appear. It is a bimonthly periodical which includes theoretical papers, reports

² The only possible exceptions are quotations from speeches of political leaders and accounts of heroic acts of particular individuals.

of research projects, broader reviews of research done abroad, critical book reviews, controversial discussions, reports of scientific meetings, etc. There has been some participation by foreign psychologists.

The number of psychological meetings appears to be on the increase. A meeting which took place in 1955 is listed only as the third "All-Union Conference on Questions of Psychology." The second meeting had taken place in 1953.³ The 1955 volume of *Voprosy Psikhologii* mentions only two other current meetings of psychologists, both staff meetings of research institutes. The 1956 volume of this journal lists two large-scale meetings of psychologists, one on the psychology of personality, attended by about 400 people, and one on the psychology of sport. There were two other meetings in which psychologists participated, i.e., one on "Defectology" (study of blindness, deafness, mental defect, etc.), attended by 600 people, and one on the scientific heritage of the physiologist-psychologist Sechenov, attended by about 300 people. There are also reports of meetings on the perception of speech, on the physiology and pathophysiology of higher nervous activity, and on the physiology and pathology of speech. In the first two issues of the *Voprosy Psikhologii* in 1957 there is a reference to a meeting on the psychology of work, and accounts of recent interdisciplinary meetings on cybernetics in relation to neurophysiology, on the work of Bekhterev, and on the "orientational reflex"; the last meeting was attended by 600 people.

Recently a Society of Psychologists was established by the Academy of Pedagogical Sciences of the RSFSR (23). Smirnov is president of the organizing committee; Leontiev, Rubenstein and Teplov are the vice presidents.

The Learned Council of the Ministry of Health recently decided to take steps to introduce the teaching of psychotherapy courses in medical schools (24) which is interesting in view of the curiously noncommittal treatment of psychotherapy in Russian books on psychiatry. There are reasons to believe that the freedom of Russian psychologists is on the increase. London has assured this reviewer that he has seen a very recent Russian psychology textbook, in which there were few (if any) references to Pavlov. A recent paper by Sokolov (25) states that a possible re-examination of psychological techniques for the study of individuals is being currently discussed.

The emphasis in this review.—The main concern in this review is not with the material covered in this introduction, but with the psychological content proper of Soviet psychology, i.e., with the picture of experience and behavior which it presents and with its research findings.

In collecting material for this review the Monthly List of Russian Accessions to the Library of Congress for 1955, 1956 and the early part of 1957 was used as the principal source of information about the available material. Books on psychology are classified in this list under the heading "Philosophy and Religion." Additional categories consulted were Education, Medicine,

³ It was not ascertained when the first meeting was held.

and Science. This review is based mainly on an examination of four recent psychology textbooks (three less recent ones were also consulted); a number of books with titles appearing to be psychological in content; four books dealing with the related field of psychiatry; the proceedings of two psychological meetings; and the periodicals *Questions of Psychology* (*Voprosy Psikhologii*) and *Journal of Higher Nervous Activity* (*Zhurnal Vysshei Nervnoi Deyatelnosti*). *Soviet Pedagogy* (*Sovietskaya Pedagogika*) and *Questions of Philosophy* (*Voprosy Filosofii*) were also consulted, but little relevant material was found.

THE VIEW OF PSYCHOLOGY PRESENTED IN RECENT RUSSIAN TEXTBOOKS

The monthly list of Russian Accessions for 1955 to early 1957 contains five Russian textbooks published in 1954 to 1956 as acquired by the Library of Congress. Only three, all published in 1955, could be located; a 1950 edition of one of the other two books was also obtained (9). All four books proved to have much in common. They had very similar chapter titles. The theoretical points of view were for the most part similar. Much of the illustrative material was repeated in several books. Psychology is defined in all of them as science of mind (*psikhika*), which tends to be used interchangeably with psychic life (*psikhicheskaya zhizn*); psychic life consists of cognition, feeling, and will. In man, it takes the form of consciousness. There is much emphasis on interactions between cognition, feeling, and will, and between consciousness and behavior. Consciousness and conscious control are treated as involved in all behavior. It is emphasized that violent emotion does not abolish consciousness or conscious control; the person knows what he is doing and remembers it afterwards, even though he may be doing something of which he might disapprove at another time. Motor performances are treated as under conscious control; a skilled performance is treated as demanding skilled perception. There are numerous references to cognitive factors in emotional and volitional processes; voluntary activity depends on anticipation of the goal, i.e., on thinking, which is a cognitive process; it also depends on perception, another cognitive process. Emotion and will are both treated as dependent on convictions. Cognition is also treated as dependent on emotional and volitional processes. Emotion is treated as participating in intellectual activity. It may be an important contributory factor in intellectual work, may result from intellectual convictions, and may be among the gratifications of the work.

There are also many references to the educability of psychic functions, e.g., of sensation, perception, attention, imagination, will, etc. Education includes self-education. What one learns depends on what one practices. It is often recommended that, in order to practice the correct actions, one should avoid situations in which the wrong form of behavior is inevitable, and that it is often desirable to begin by acquiring a clear conception about the performance one tries to learn.

In discussions of emotions, the will, and personality, there is much utilization of concepts which appear to have been chosen in terms of the

value of the traits for society—traits like devotion to theoretical principles, ability to show initiative, tendency to act upon one's convictions.

Much credit is given to Pavlov, particularly in the newer textbooks,⁴ for investigating the laws of higher nervous activity. There are many references to his ideas of formation of "temporary bonds" or conditional reflexes, of synthesis and analysis as aspects of brain activity. Sensation is treated as a product of analysis. Conditional reflexes are a product of synthesis involving two cortical processes. There can be "associations between associations." Sequences of conditional reflexes are apt to develop into complex "stereotypes," which are likely to be influenced in their functioning by each other and by other factors affecting the organism and which are therefore dynamic stereotypes. Pavlov's concept "temporary bond" is obviously capable of being utilized (and has been utilized) in explanations of sensory integration. Conditional reflexes are responses of the organism to signals of coming events. They form a system of signals. In man there is a second signal system, a set of signals of signals. This second signal system is speech. Pavlov described conditional responses to relations between stimuli. He viewed the different sensory systems as overlapping in their cortical representation and objected to the notion of rigid and neat separate localization of different functions in the brain. He coined the term "systemness" (*sistemnost*) in order to characterize the integrated nature of brain functioning. According to Asratian (26), he emphasized the unity of the organism as early as 1903. These aspects of Pavlov's theories have points of contact with gestalt theory; they are often emphasized in contemporary Russian psychology and physiology.

Pavlov distinguished between excitatory and inhibitory processes in the brain. Both are aspects of the maintenance of equilibrium by the organism. Initially, foci of excitation or inhibition tend to spread or irradiate; after a while they tend to concentrate, and a focus of excitation induces inhibition in the surrounding area (negative induction). If the stimulation is too intense, and under certain additional conditions, the normal quantitative relations between stimulation and excitatory or inhibitory processes are apt to be disturbed; in extreme cases, normally excitatory stimuli may lead to inhibition and inhibitory processes to excitation (ultraparadoxical phase).

The substitution of inhibition for excitation when stimulation becomes too strong protects the brain from over-strong excitation. These views of Pavlov are presented in the psychology textbooks but only a few of their potential applications are utilized. Negative induction is mentioned in discussions of attention; but both negative induction and protective inhibition could also be utilized in explaining much the same facts for which the concept of repression was invented. Some of these applications of the Pavlovian ideas occur in the Russian psychopathological literature, but not in the psychology textbooks.

The four psychology textbooks will now be described. One of them, that

⁴ Some of the parts of Pavlov's teaching mentioned here come from sources other than the textbooks.

by Teplov (9), is a textbook for secondary schools. According to a review in *Voprosy Psikhologii*, Ivanov's book (11) is intended for teachers' colleges. Egorov's book (12) was written for military academies; Rudik's book (10) is for use in schools of physical education. None of the books has an author index or a subject index. There seem to be no references to controversies in Soviet psychology and there are only a very few references to specific experimental studies, no lists of references and hardly any tables or graphs. There is a tendency to use literary examples as illustrative material. In view of the great similarity between the chapter titles of all four books, those of only one will be presented. The chapter titles of Teplov's book are: (a) Subject matter, tasks, and methods of psychology; (b) Development of psychic life; (c) Sensations; (d) Perception; (e) Feelings; (f) Attention; (g) Memory; (h) Imagination; (i) Thinking and Speech; (j) Will; (k) Psychological characterization of activity; (l) Psychological characterization of the person. The other three books have 15 chapters each. Most of the chapter titles are similar to Teplov's; the larger number of chapters is almost entirely due to the fact that in some instances the material covered in one chapter by Teplov is subdivided into two or three chapters by another author.

The definitions of psychology which are formulated in terms of psychic life and consciousness have already been mentioned. Cognitive processes are treated as reflections⁴ of reality. Sensation "reflects" separate aspects of objects and events. It is a product of analyzing activity of the sensory mechanisms which begin with the sense organs and include the primary sensory cortical areas and which are termed "analyzers" in accordance with Pavlov's usage. Sensory cortical processes are synthesized in secondary areas into perception or reflection of whole objects and events of the world. Perception also depends on past experience and on temporary moods. Herbart's old term apperception is used in describing these influences. Perception is influenced by attention, which is viewed as based on mutual induction of cortical excitation and inhibition. The past is reflected by memory, which sometimes utilizes imagery. Imagination also utilizes imagery; it recombines images of past experiences into new wholes. Distinctions are made between creative imagination, re-creative imagination (e.g., imagining a landscape on the basis of a map or a description), realistic daydreams leading to activity, and useless daydreams unrelated to action. Thinking is viewed as reflection of general and essential properties of objects and events, of relations between them, and as indirect cognition of the world.

⁴ According to London, the term reflection comes from a paper by Lenin, and its wide use represents Lenin's influence. It is hard to determine exactly what reflection is intended to mean in this context. A mirror reflects images. A person's development reflects the influences which have helped to shape him. These same two different meanings of reflection are both applicable to its Russian equivalent; the Russian psychologists do not discuss it in detail.

Thinking is treated as closely related to speech and as always involving speech in some form, but not as identical with speech. Thinking makes much use of concepts, which are treated as products of abstraction and as different from images. Concepts and relations between them are expressed in speech.

Historically, these views are related to the Würzburg school. In Rubinstein's book, published in 1940 (27), the work of the Würzburg school is mentioned, the relation between images and concepts is discussed, and reasons are given for treating them as different from but at the same time generally interpenetrating each other. In the newer texts the discussion is on a more elementary level, so that it may lead to the inaccurate impression that the treatment of the term "concept" is based on an uninformed confusion between logic and psychology.

The terms "feeling" and "emotion" are used as synonyms. Feelings reflect one's relations to oneself and the world. They involve subcortical centers and the autonomic nervous system. They tend to be characterized by polarity. Most of them can be arranged in pairs of opposites, e.g., pleasure-displeasure, love-hate, excitement-rest. The most important pairs are pleasure-displeasure and activity-passivity. The latter pair leads to a general distinction between sthenic emotions which give strength and promote activity, and the asthenic emotions which interfere with activity.

Generally speaking, the attitude toward emotion, particularly sthenic emotion, tends to be more favorable than in many American textbooks. The potential significance of emotion in intellectual and artistic creativity, in effective social action, in heroic conduct in war, etc., is pointed out repeatedly. However, emotion may be passively experienced without leading to action; an important dimension of emotion and its individual peculiarities involves the degree to which it tends to lead to appropriate action rather than passive experiencing. The degree to which emotion depends on theoretical convictions is another variable; in some people it does to a great extent, while in others it depends largely on the immediate situation. Emotion depends on the mood; a mood may begin as a result of an event which is barely noticed at the time and which may be quickly forgotten. Thus a person need not be aware of all factors determining his emotions.

The will is the side of psychic life which expresses itself in conscious goal-directedness of actions. A more restricted meaning of the will refers to the overcoming of internal or external obstacles.

Human action is viewed as determined by motives and as directed toward goals; different degrees of consciousness of the goal of action are pointed out, but all action is viewed as more or less voluntary. Involuntary movements, e.g., arm moving while walking, are not treated as actions.

Motives are derived from needs. Neither the motives nor the needs are discussed systematically, but a distinction between material and mental motives is mentioned. Among the mental motives, the sense of duty and the tendency to evaluate oneself morally (conscience) are mentioned. Con-

science and sense of duty presuppose convictions.⁶ Conflict generally tends to involve the sense of duty.

The principal traits of people, so far as their wills are concerned, are: (a) the degree to which actions are determined by conscious convictions (this desirable trait has two opposites, i.e., negativism and suggestibility); (b) the ability to arrive at decisions; (c) self control; (d) energy and persistence.

Chapters on "Psychological analysis of activity" are apt to contain discussions of the relations between activity, two kinds of habit formation (habit as practiced skill and habit as acquired need—*navyk* and *privychka* respectively),⁷ creative effort, and motivation. Most of the materials are familiar; but there are some discussions in which conscious control of activity is emphasized to a greater extent than in non-Russian psychology. In the case of acquisition of skills the advice is given to begin with a clear idea about the correct performance. In Rudik's book it is stated that reports of outstanding athletes indicate that highly practiced performance does not mean abolition of conscious control but may mean its redirection; the person may become less conscious of the details of the movement but more conscious of its outcome. The skilled athlete may also perceive his movements and the relevant situation in a particularly full and detailed manner.

Discussions of differences between people generally emphasize the idea that only anatomical and physiological characteristics of people can be inherited. The psychological characteristics of people are products of development within an environment, generally a social environment. They are never completely fixed. Depending on the physiological makeup, some people can acquire certain abilities or aptitudes or other traits earlier or more easily than others. Aptitudes do not function singly but in patterns. Different people may achieve the same results by means of different aptitudes; the lack of one aptitude may be compensated for by another. Practically everybody has a pattern of aptitudes enabling him to be gifted in some field. Interests are treated as among the most important personality characteristics. It is pointed out that they develop out of needs and are related to convictions. They differ in breadth, in depth (or degree to which they are related to central issues in the life of the person), in stability, and in their power to produce activity.

In all of the textbooks which were examined there was a favorable account of Pavlov's theory of different types of nervous functioning and resulting different temperaments. According to Pavlov, nervous systems of

⁶ This affords the authors the opportunity to state that the correct convictions are the Marxist-Leninist ones.

⁷ A third concept, that of ability to perform or know-how (*umenie*—sometimes also translated as skill) plays a role in recent publications. A *navyk* requires practice for its formation. An *umenie* may be based on a verbal explanation or a demonstration. An active controversy about these concepts and their relations to other concepts, e.g., those of dynamic stereotype, set, etc., which is in progress, will be briefly characterized later in this review.

different people (or of different dogs, or other animals) differ in their ability to tolerate strong stimulation and excitation without disruption of their functioning. Pavlov thought also that "strong" nervous systems could differ in their balance between excitation and inhibitory processes. Nervous systems with evenly balanced excitation and inhibition could be relatively "mobile" or "immobile"; in a "mobile" nervous system, excitation and inhibition tend to alternate; in an "immobile" one they tend to persist. Pavlov thought that his "strong unbalanced excitatory" type had a resemblance with the Hippocratic choleric type; the weak type with the melancholic temperament; the strong balanced mobile and immobile types were thought to resemble the sanguine and phlegmatic types respectively. In addition to the above types viewed as common to man and animals, Pavlov thought that people could also be classified in accordance with the relative dominance of the first signal system (that of the senses in general) or the second signal system (that of speech).

The Pavlov doctrine of the types of nervous activity is presented more fully in the textbooks which appeared in 1955 than in the book for high schools published in 1950. The different "types" are treated as of equal value. In this they depart from Pavlov and follow Teplov (see later). Their existence requires that different people should be treated differently, e.g., in educational situations.

The expression "character" has a broader meaning in Russian than in American psychology; it corresponds approximately to that of "personality." It includes relations to others (e.g., the traits of sociability, firmness, and politeness), to the self (e.g., the traits of self-esteem, modesty, egocentricity, and selfishness), and work (e.g., initiative, reliability, persistence). It also includes traits like devotion to principles, honesty, discipline, etc. In three of the four books which were examined there were also sections on the traits said to be typical of the new Soviet man. These discussions of the "new Soviet man" have been already characterized in the introductory part of this paper.

There are a number of topics that are strikingly conspicuous by their absence from the Russian textbooks. First, there is virtually no technical information about techniques of study of individual differences. The method of tests, e.g., of devices whereby one obtains quantitative scores for human performances, generally in relation to statistically derived norms, is either ignored or criticized. The criticism is chiefly based on the fact that tests usually fail to specify the processes which result in a given performance. There is also criticism of the alleged belief, held by unnamed bourgeois psychologists, that tests are wholly measures of innate capacities. It is sometimes implied in textbooks (e.g., Rudik's) that experimental research on individual differences (as distinguished from the test approach) is legitimate, but no information about the techniques of such research is presented.

Another topic which is not presented in any detail is that of motivation. There is no systematic discussion of needs. Very little is said about acquired

motives; the nature of the mechanism leading to their acquisition is not discussed, beyond statements asserting the importance of education, self-education, and the formation of convictions. The role of social factors in motivation and personality development is acknowledged and emphasized, but very little factual material is presented. There is no anthropological material. There is practically no information about the role of the family in the development of motivation and personality and there is little information about conflict and frustration—none from psychoanalytic sources.

Additional books.—A number of books were consulted in addition to the psychology textbooks: a textbook and two books of lectures on psychiatry [Giliarovsky (28); Kerbikov (29); Sukhareva (30)]; a book [Ravkin (31)] on out-patient treatment of mental disorders; a small popular book [Gotovtsev (32)] about the nature and treatment of enuresis. These books on psychiatry were consulted mainly to investigate further whether ideas of psychoanalytic origin had any influence in Russia.

The books on psychiatry were found to emphasize physiological factors in mental disorders. Much theorizing, along the lines suggested by Pavlov late in his life [Pavlov (33)], utilizes ideas such as disturbed balance between the cortex and subcortical centers and persistent foci of excitation which disturb brain functioning by the mechanism of negative induction. Much information is presented on possible organic factors in mental disorders. The existence of psychogenic disorders is acknowledged, but very little is said about them. Case histories are apt to be very sketchy; as a rule, no information about the patient's childhood is presented. Some ideas of psychoanalytic origin are mentioned, but in a changed context. Thus schizophrenic emotional withdrawal is apt to be mentioned, but not as a basis for other symptoms but simply as one of the symptoms of the disorder, to be noted for the purpose of differential diagnosis. Psychotherapy is mentioned, and there are statements to the effect that there are conditions for which it represents the most promising form of treatment. There are references to psychotherapeutic friendly conversations (*psikhoterapevicheskie besedy*) in recent Russian psychiatric writings. But no information is given about the technique of psychotherapy or the content of the psychotherapeutic conversations. This absence of information is particularly striking because of the amount of detail about other methods of treatment, particularly those in which drugs are used. In view of this striking absence of detail about causes of psychogenic disorders and about psychotherapy, this reviewer wonders whether the absence of psychoanalytic influence in the Soviet Union is as complete as it seems to be. Freud is not unknown in Russia. The Soviet historian of psychiatry, Kannabikh (34), referred to him very favorably. According to Wortis (35), there was a well-informed and largely favorable article on psychoanalysis in an earlier edition of the Great Soviet Encyclopedia, and there was a psychoanalytic society in Russia in the early 1930's.

Gotovtsev's small book on nocturnal enuresis (32) in children presents the Pavlovian idea that sleep represents widespread cortical inhibition.

Normally, there are guarding mechanisms which enable the sleeper to wake up in response to certain signals which indicate the need to wake up. But the enuretics sleep too deeply; the author states that there are experimental findings which have demonstrated this fact. The author views enuresis as due to failure to wake up upon the bodily signals of a full bladder. He views the too-deep sleep as resulting from disordered nervous functioning such as is likely to develop in an unhappy home. He recommends a method of treatment involving partly improving home conditions and partly enlisting the child's cooperation in training to wake up at specified times.

A book by Hackel (36) deals with sleep, dreams, and hypnosis. Its chief purpose seems to be to counteract superstitious beliefs. The Pavlovian view of the nature of sleep is presented. Dreams are viewed as representing partial sleep; the second signal system and the function of criticism are particularly likely to be inhibited. Uninhibited functions are likely to include certain foci of excitation which are started by external stimuli or which represent wishes. The normal intensity relations between stimulation and experience are disturbed; phenomena characteristic of Pavlov's "ultraparadoxical phase" take place. The occurrence of remote memories in dreams is such a phenomenon. Hypnosis is treated as related to dreams, i.e., also as a state of widespread cortical inhibition, but with some cortical mechanisms still functioning.

Kovalev's book on personality (37) contains accounts of Marxist views of personality and Pavlovian ideas about types of nervous functioning and conditional reflexes. The ideas of the Soviet educator Makarenko on the role of social collectives in education are often mentioned. Much of the book is educational in intent, and a number of educational case histories are presented. Personality is regarded as developing through social interaction, and the person is viewed as tending to become what he practices. Thus a person becomes honest if he grows up in situations in which it is easy to behave honestly and he does not have to cheat.

Yakobson's book on emotions (38) contains material of the type found in chapters on emotions in Russian psychology textbooks, with additional material on the physiology of emotions. It devotes much space to the education of emotions and to Makarenko's ideas on the utilization of collectives (e.g., class meetings) in emotional education.

A book by Chernikova on the psychology of sports (39) contains material on the experiences of performers, what demands are placed upon them, and how they learn to meet the demands. Thus it is pointed out that a good athlete may have to learn to act decisively, to overcome fear, to estimate his pace very accurately, etc. The great persistence of outstanding athletes and their frequent interest in the theory of their sport are discussed. Some experimental evidence is presented which indicates that outstanding performers in certain sports tend to have very fast reaction times. The author apparently does not consider the possibility that the results may have been produced by selective factors rather than by training.

A small book by Blatin on "Friendship and Comradeship" was also con-

sulted but was found political and moralistic in content rather than psychological.

THE 1953 CONFERENCE

The published proceedings of two of the psychological meetings were examined. In addition the reports of one of these and of some additional meetings, as presented in the *Voprosy Psikhologii*, were also consulted. These reports tended not to contain much concrete information about the content of the papers, but they did indicate that the impression of near-unanimity created by the textbooks does not apply to Soviet psychology in general. There were many references to both favorable and unfavorable comments about the papers and to debates among the participants. The proceedings of the 1953 meeting (40) included 31 reports. The largest category involves research on educational techniques.

Research on education.—Menchinskaya (41) presents examples from school situations which indicate that dealing with the instructional material in visual terms may bring both advantages and disadvantages. Thus the understanding of a story by a child may be improved if the child is asked to draw a picture about the story and to invent a title for the picture. On the other hand, the standardized positions of particular figures in geometry textbooks often interfere with the solution of geometric problems presented in non-standard positions.

Samarin (42) presents facts which indicate that school children do poorly when required to arrange the names of a number of cities from North to South, or to arrange such magnitudes in order of size as the length of a certain mountain range, the altitudes of certain high mountains, the length of a river, etc. Apparently the children had failed to arrange the facts learned separately into an orderly system, and the author offers suggestions on how this may be accomplished through classroom instruction.

A paper by Shardakov (43) summarizes several separate investigations of the development of thinking of school children in relation to certain concepts in different school subjects. Some of these concepts were those of causation in history; of functional relations in mathematics; and of the distinctions between causal, temporal, and conditional clauses in grammar. The author discusses the characteristic difficulties of children with these concepts (e.g., children, particularly younger ones, tend to overrate personal factors in historical causation) and suggests that different school subjects make different contributions to the thinking skills of children.

Turpanov (44) investigated the educational effects of visual and verbal methods of instruction in geography; the three lessons had to do with volcanoes, Niagara Falls, and icebergs. Individual differences were found in responses to visual and verbal instruction, and also in the ability to express knowledge in words or by drawing pictures. The author suggests that combined visual and verbal instruction should be used, partly because of individual differences and partly because of the desirability of interpenetration between imagery and conceptual thinking as an educational objective.

Dobrynin (45) found that the use of live flowers as illustrative material in botany lessons created interest in pupils, but was not as effective educationally as combined use of flowers and prepared diagrams. The latter method was superior both in relation to the flowers originally used and in transfer to other flowers. Simple statistics are presented.

Korman (46) found that retention of material learned in a course in psychology and contradicting commonly held beliefs is not perfect. Some students (generally less than 50 per cent) revert to the popular beliefs. The author suggests that the popular beliefs should be explicitly criticized in class.

Slavina (47) and Galperin (48) reported research on the learning of arithmetic by children, and on the utilization of the results in remedial instruction. Slavina worked with children who were markedly deficient in arithmetic. She found that these children tended to be intellectually passive in class and to lack basic numerical skills. They knew how to count, but did not know that adjoining numbers in the number sequence differed by one unit, or how to count backwards. Educational play sessions (from five to ten) were successfully used to counteract educational passivity. In an effort to overcome the lack of basic skills three-stage remedial methods were used. In the first stage the children performed real actions with real objects. In the second stage they had to proceed orally with imagined objects. In the third stage they had to learn to proceed mentally. Twenty to 25 remedial sessions are reported to have been sufficient, and a follow-up indicated that the children had no further trouble in arithmetic. Galperin reported similar results; he also reported material having to do with additional dimensions of mastery of arithmetic by children.

Morozova (49) investigated instructional stories as techniques for the creation of interest in a scientific procedure in school children. The stories were about explorers lost in unfamiliar localities. The scientific procedure had to do with methods for the determination of position used by the explorers. The author used several versions of the stories in an effort to discover how to direct the pupil's interest to the scientific content of the story rather than to hardships, heroism, etc. She found that emphasizing the scientific theme by explicit formulation of the corresponding question in the text had only a slight effect on the direction of the children's interest, and that assigning the task of discovery of location to a young hero had a much greater effect, the more so the more central was the problem for the hero. It is suggested by the author that there are two main directions of development of interest in children, from personal to impersonal and from concrete to abstract.

The common emphasis in the educational papers appears to be on the aim to achieve understanding in the student, which is viewed as developing out of the combined effects of verbal and perceptual material, or of relatively concrete and relatively abstract material. Similar themes occur in several additional papers which do not directly deal with educational problems.

The effects of verbal directions and verbally presented information.—Zankov

& Petrova (50) report experiments on the effects of the verbal instruction "try to remember" on the retention of sentences by normal and by feeble-minded subjects. They reported that in normal subjects the verbal instructions resulted in a much greater superiority of intentional memory over incidental memory than in the feeble-minded subjects. The examples presented by the authors suggest that a rather substantial component of this result had to do with the better incidental memory of the feeble-minded.

Luria (51) reported on the ineffectiveness of verbal instructions in feeble-minded subjects in another learning task. He claims that normal two- to three-year-old children cannot follow consistently the instructions to respond to one kind of light by pressing a button while ignoring another kind of light. They can learn the task by the technique of "conditioning with verbal reinforcement," in which one light signal is regularly followed by the command "press," the other by "don't press." The older feeble-minded children⁸ could not perform the task, either in response to verbal instructions, or as a conditioned response with verbal reinforcement, even if they correctly recalled the verbal instructions. They also failed some additional tasks. The author interprets the results as meaning that the normal relation between the two signal systems⁹ is disturbed.

Zaporozhets (52) reported that verbal directions tend to result in faster and more conscious learning and more generalized effects than teaching by demonstration, provided that the child is ready to follow directions. For complicated tasks, demonstrations are apt to work better than verbal directions, because readiness to follow directions develops later in life with complicated tasks than with simple tasks. More or less similar findings, indicating a gradual development with age of ability to follow verbal instructions, resulting in faster learning and better transfer of training, were also reported by Neverovich (53), Lublinskaya (54), and Zinchenko & Kontsevaya (55). Neverovich also reported favorable effects of watching the experimenter's actions in demonstrating a task to the subject, and, in another experiment, the beneficial effects of preliminary examination of a maze through which the subjects had to learn to push a toy truck blindfolded. In Zinchenko & Kontsevaya's paper there is additional material. There are several discussions related to the reinforcement concept; in one of them the solution of a problem is treated as a reinforcing factor, and it is pointed out that Pavlov accepted this as one of the interpretations of reinforcement. Elkin (56) reported on experiments demonstrating the influence of verbal information on a conditional withdrawal reaction; tachistoscopically presented geometric figures were the conditional stimuli. The development of the conditional response appeared to be speeded up if the subject was told beforehand which one of several figures was to be followed by the shock. It was slowed down if the subject was misinformed about the figure. Elkin also found that the

⁸ No information about the degree of mental defect is given.

⁹ The perceptual ("first") and the speech ("second") systems.

conditional response tended to appear on the same trial in which the subject first perceived and described the figure correctly. According to the author this means that perception is essentially a process of the same type as conditional reflexes.

Papers on Pavlovian typology.—Another group of papers deal with Pavlovian typology of nervous functioning. Three of the papers are by Teplov and by his collaborators, Kossov and Borisova; since the time of the meeting a book by Teplov and his collaborators and additional papers have appeared on the same subject.

Teplov's paper (57) treats different types of nervous functioning, including those sometimes viewed as inferior, as potentially equally valuable. This point is elaborated in his later publications (58, 59) in which he suggests that, for example, the "weak" type of functioning may have the advantage of low thresholds of sensitivity. The author also suggests that the type of nervous functioning need not be the same for different sense modalities (analyzers) of a given person. The paper advocates using involuntary activities, rather than voluntary acts or life-history data, for studying the type of nervous functioning; with involuntary activities, the type of the nervous system may be less masked by acquired temporary bonds.

Two methods for the study of types of nervous functioning are presented in Teplov's paper. One makes use of Dolin's "conditional photochemical reflex" or conditional disturbance of dark adaptation; the unconditional stimulus was the turning on of a light bulb in front of the dark-adapted subject. Maisel, working in Teplov's laboratory, determined the rates at which the phenomena of conditioning, extinction, and differentiation developed in different subjects and the results were used as measures of the balance between excitation and inhibition. Contrary to Pavlov's opinion, a preponderantly inhibitory type was found to exist. Its existence was postulated if extinction and differentiation took place considerably faster than conditioning.

The technique for the study of the "strength" of the nervous system involved the Pavlovian "caffeine test" in relation to the alteration of the visual threshold by a weak extraneous stimulus elsewhere in the visual field. Such a stimulus, if weak enough, tends to lower the threshold as a result of irradiation of excitation. But if the sensitivity of the nervous system is increased by a sufficiently large dose of caffeine the effects of the extraneous stimulus are likely to concentrate rather than irradiate, thus raising the threshold for the other stimulus. However, with a further increase in sensitivity the nervous processes tend to irradiate again, which again lowers the threshold. From these considerations, the author deduces that for the "strongest" nervous system caffeine should leave the interaction between the stimuli unchanged; for somewhat weaker nervous systems caffeine should transform the lowering of the threshold into the opposite effect or a raising of the threshold; while for the weakest nervous systems larger doses of caffeine should retransform the effect of extraneous stimulation into lowering

of the threshold. The experimental work on these effects, reported by Teplov, was done by Rozhdestvenskaya, whose fuller account of it has since appeared (60).

In the experiments by Kossov (61) an attempt was made to determine the relative dominance of the first and second signal systems in individuals. For this purpose, subjects were trained to react (or not to react) to sequences of paired stimuli; one member of each pair was always a colored light, the other a color name. In the test trials positive sequences of lights were combined with negative sequences of color names and vice versa. Some subjects were found to respond consistently in accordance with the light sequence ($n=6$), some responded to the name sequence ($n=10$), some responded differently in different trials ($n=6$), and some did not respond at all. Additional experiments indicated that differentiation between positive and negative sequences of stimuli tended to develop faster for the dominant signal system.

In a study by Borisova (62), a comparison between two tests of memory for visual shapes was used to investigate the relative predominance of the two signal systems. The visual material consisted of drawings of maple leaves or butterflies. In one method, the subject had to look at one drawing and pick out the same one from among ten on a page. In the other method, the subject was asked to look at a page with the drawings and then describe one of them so that another person could find it. Some subjects did better with the first task, some did better with the second task, some did about equally well (or poorly) with both. The method of scoring is not described. Doing better with the first task was interpreted as indicating predominance of the first over the second signal system; doing better with the task of verbal description was thought to indicate the opposite. Of 34 art students who were tested, 23 did better on the first task, 11 equally well on both; none showed preponderance of the speech system.

In experiments by Saprykin & Milerian (63), the "type of nervous functioning" was studied by a task involving learning to press colored buttons in response to rapidly appearing lights of the same colors. When the subject learned the task at a given rate of speed, the apparatus was speeded up and the subject had to learn to adjust to the higher speed. This was repeated several times. In additional experiments the sequence of the lights or the position of the response buttons was changed. In still other experiments certain buttons were not to be pushed. The authors interpret various results of their experiments as measures of excitation, inhibition, balance, and mobility of nervous processes. The authors claim to have found evidence of the four Pavlovian types in their data. But their apparently discontinuous distribution was based on only 27 cases, and seven of them did not fit into any of the four types.

Experimental papers on Pavlovian "neurodynamics."—Two more papers in the Reports at the Conference are devoted to Pavlovian "neurodynamic" concepts. Boiko (64) and his collaborators investigated the sequence of reac-

tion times to lights distributed over a panel. The time and spatial distance between consecutive stimuli varied. The concepts of self-induction, irradiation, and concentration with negative induction were used in predicting the effects of the earlier reaction times on the later ones. Over a time interval, the effect was expected to change from slowing down to speeding up to slowing down again. These effects were expected to appear earlier in the case of stimuli located near each other than in the case of distant stimuli. The results confirmed the expectations.

Bolshunov (65) suggested that negative induction might account for retroactive inhibition and that the disappearance of negative induction may be the cause for the phenomenon of reminiscence. In order to test this theory the author conducted experiments in which subjects had to learn sequences of flashing lights and reproduce them by pressing appropriate keys. In the reminiscence experiments the subjects learned the sequences to from 60 to 80 per cent mastery and then continued consecutive unaided reproductions until these became stabilized. The subjects' performance generally improved during these reproductions and this improvement was treated as reminiscence. It was greatest in the case of the sequences where the opportunities for negative induction were thought to be greatest, namely with the sequences with small separation in time and space between the lights. Similar considerations led to experiments with two consecutive sequences of lights which were used to test the author's theory of retroactive inhibition.

Experiments on thinking.—Two papers deal with the psychology of thinking. Leontiev (66) reports experiments on the solving of spatial problems (conducted by his collaborators Ponomarev and Hippenreiter). It was found that "leading towards" problems which had common features with the experimental problems generally failed to help the subjects if they were presented first; they were likely to help if the experimental puzzle was presented first, then the "leading towards" problem, then the puzzle again. But the effectiveness of the leading problem depends on sufficient interest of the subject, and on an appropriate degree of effort exerted in the original attempt to solve the puzzle. The author characterizes the process of discovery of the solution as a "locking" of a Pavlovian temporary connection.

In Ramishvili's study (67) subjects were asked to define a number of common concepts, e.g., "grass," "gardens," "justice." They were also asked questions of the type "is a potato field in bloom a garden?" It was found that the subjects generally agreed with each other about the meaning of the words, as shown by the high consistency of their answers to questions about what was or was not a garden. However, the definitions suggested by the subjects (mostly people with higher education) usually failed to agree with this common usage, and the subjects were generally not able to replace them with satisfactory ones after the discrepancies were pointed out to them. The author concludes that the meaning of concepts, as it operates in thinking, is not fully conscious and not readily formulable, but is a set.

Research on speech.—Two papers, by Artemov (68) and by Zhinkin (69)

deal with speech. Artemov's paper is a review of a large number of studies of different aspects of both spoken and written speech, ranging from research on how foreign language sounds can best be taught to how form and content contribute to the impression made by works of literature. Zhinkin's paper is a review of studies dealing with the physics and physiology of sound production in speech. The relative contributions of mouth and throat movements were studied. One finding was that both mouth and throat resonate so as to reinforce both high and low partial tones, and that the relative contributions of mouth and throat to high and low partial tones differ in the case of different vowels. Another finding indicated that the mouth movements of deaf speakers are relatively normal, but their throat movements are markedly defective.

Sensation and perception.—The remaining five papers deal with various aspects of sensation and perception. Schwartz (70) attempted to lower a measure of visual sensitivity by training. The visual task was the identification of the orientation of E, which was presented in four different positions. A threshold-like measure was defined in terms of five successive correct recognitions. Four different training techniques were used: simple repetition, being told the correct position after each response, a weak electric shock after each error, and information about the amount of improvement already achieved together with an assignment of specified further progress. Simple repetition resulted only in a slight improvement; the method of information and assignment was the most effective one of the four methods and is reported to have resulted in reductions of the measure of sensitivity in a ratio of better than 10 to 1. Evidence of transfer to other shapes is presented.

In studies by Natadze (71) and Bzhalava (72), Uznadze's illusions of size contrast were investigated: a subject is apt to perceive two spheres of equal size, one on the right side and one on the left side, as different, if they are preceded by repeatedly presented unequal spheres.¹⁰ The illusion is reported to apply both to objects presented visually and objects perceived by the hand, and intersensory effects are also reported. In Natadze's study, the question is investigated whether the illusion, when perceived by the hand, is defined in terms of the hand, in terms of the side of the body, or in relation to the experimental room. Experiments are reported in which the unequal balls are perceived by crossed hands and the illusion is tested by hands in normal positions; various other combinations of crossed and normal positions were tried out. In most of these combinations, the illusion was found to "travel with the hand." But there was one combination in which the illusion was specific to the experimental setup rather than the hand. In this combination, the unequal balls were hidden in two boxes on a table with many objects; the subject was asked to put his hands one into each box, and feel the balls. In the test trials, the subject had to place his hands into the boxes from the other side of the table.

¹⁰ If the difference between the original spheres is small, the subsequent illusion tends to be of assimilation rather than contrast.

In Bzhalava's study, successive size contrast is demonstrated in the case of visual after-images. It is shown that successive size contrast may be conditioned by combining the presentations of unequal circles with sounds. After-images may also be conditioned.

A study of Hojava (73) deals with a long-known finding of Ranschburg: when a number of letters are presented tachistoscopically and include two equal ones, more errors are made in reporting these equal elements than the others. Hojava suggests that the effect is due to the set to perceive different items, and reports an experiment in which such a set is established by presentation of "all different" combinations. If a combination with a pair of identical items is then presented, the number of errors is much increased.

A study by Sokolov (74) is a part of a large project dealing with physiological changes in the body during perception, or the "orientational reflex." In this paper, vasoconstriction as demonstrated by a finger plethysmograph in response to sounds is investigated. The author views it as a part of the process contributing to the dealing of the organism with the stimulus, because if the stimulus is weak and the subject is not asked to respond to it the vasoconstrictive reaction soon disappears. But it persists if the subject is asked to press a button when the sound begins and release it at its end. In general, the vasoconstrictive reaction increases with the intensity of the stimulus. But this trend is reversed near the absolute threshold; the very weak stimuli, which can only be perceived with effort, produce large plethysmographic responses. Even subliminal stimuli to which the subject does not react overtly may produce plethysmographic responses.

THE 1956 CONFERENCE ON PSYCHOLOGY OF INDIVIDUALITY

The proceedings of the Conference on Questions of Psychology of Individuality (75) appear to have been published only in an abbreviated form, and many of the papers are too condensed to be reviewed; in a number of instances it is not even clear whether the points made by the authors represent theoretical conjectures or are based on particular facts. A number of papers will not be reviewed, some for this reason, others as dealing with the program for a psychology of personality in the light of Marxism.

The papers which will be reviewed fall into several categories. A number of papers deal with Pavlovian typology, suggesting that there is considerable interest in this topic in the USSR.

Pavlovian typology.—A paper by Teplov & Leites (59) presents much the same ideas as were presented by Teplov at the 1953 Conference. A paper by Krasnogorsky (76) presents a view of types, different from that of Teplov, in which the weak type is viewed as relatively unexcitable. A paper by Ilyina (77) presents a comparison between the social reactions of two children of different "types" (as determined by a technique based on the galvanic skin reflex), one "sanguine" and one "phlegmatic." Both children were actively sociable; but they differed in the expected direction in speed of establishing initial social contacts, duration of social contacts, number of friends, etc.

Malanova (78) investigated records of speech by six women, three "typed" as excitatory, three as inhibitory on the basis of a procedure involving their life history and conditional eye wink reflex. There was no difference in the over-all speed. But the speech of the excitatory subjects was less even and more expressive than that of the inhibitory ones. A paper by Palei & Penskaya (79) points to methodological difficulties which arise if one attempts to use the conditional galvanic skin reflex in studying the type of nervous functioning. The results are too variable. Too many factors enter into the situation, including uninstructed inner verbal reinforcement. A modification of technique is described as giving more regular results. Elkin (80) suggests a technique for the study of types of nervous functioning which is based on association experiments. The course of the response time in a free association test is viewed as an indicator of the degree of balance between excitation and inhibition. A stable response time means balance between these processes; increasing response time means preponderance of inhibition; decreasing response time indicates preponderance of excitation. The author states that the conclusions reached about the excitation-inhibition balance of individuals were confirmed by the results of a second experiment in which the subjects were instructed not to respond to names of plants; the "excitatory" subjects made errors of commission, while the "inhibitory" made errors of omission. A third and fourth experiment consisted of two parts each; experiment 3 was a free association experiment, experiment 4 a controlled association experiment; in the second part of both experiments the subject had to respond to his own responses given in the first part. Doing better on the second part of these experiments than on the first part was viewed as indicating mobility of nervous functioning.

Papers on abnormal psychology.—A number of papers deal with problems of abnormal psychology, or what would be considered abnormal psychology in America. Eres (19) studied forty-five behavior problem children ("undisciplined") by means of interviews, observation, examination of records, etc. There was no control group, and no statistics are given. The causes of the behavior problems appear to have been: (a) parental neglect, little supervision, (b) indulgent overprotection; (c) parental despotism, rudeness, quarrels, (d) low moral standards of the family; (e) parental inconsistency, ignorance of methods of good upbringing, so that parents with good intentions were baffled.

Zachepitsky & Yakovleva (81) attempt to characterize the typical personalities, conflicts, and methods of upbringing contributing to hysteria, neurasthenia, and psychasthenia. They view neuroses as resulting from psychic traumata, but only those involving significant human relations. Hysteria is viewed as developing in people who received too much attention as children, or who were frustrated too much. In either case, the ability to overcome obstacles does not develop properly, and the typical conflicts of hysterics involve environmental demands. Neurasthenia is apt to develop where there have been excessive demands on the child. Exaggerated demands

result in a weakening of inhibitory processes and in irritable weakness. The typical conflict in neurasthenics is between their own demands and their resources. Psychasthenia is likely to develop after the child's independence has been suppressed and the child has been protected from activity. The typical conflict is between contradictory inner impulses.

Psychotherapy is characterized as a re-educational process also involving neurosomatic strengthening. The conclusions of the study are said to be based partly on observations in Miasishchev's clinic and partly on a variety of laboratory procedures, including association experiments and work with electroencephalograms and galvanic skin responses.

Miasishchev's paper (82) appears to be mainly a theoretical article making use of Marxist ideas, but also influenced by clinical work. It points out that personality development is affected by mutual relations. Early experiences with people determine the readiness to accept unpleasant tasks and to refrain from desired actions. Consciousness of duties and of the forbidden; and traits like ability to show initiative, bravery, and frankness; shut-in character; cowardice, etc., develop out of such experiences. Family, school, and work collectives all contribute to socialization. Too little frustration is likely to lead to individualism and insufficient socialization. Excessive demands and too little gratification are likely to lead to hypocrisy, lack of distinctive individuality, and neurosis. Normal development is apt to involve a change from being influenced by people to being influenced by principles. The author regrets that the role of the developing sex drive in young people is a topic which is not being discussed.

Averbukh (83) discusses personality alterations in psychopathology. Organic disorders, particularly those of vascular origin, are apt to resemble neuroses in their early stages. Many inhibitions and convictions weaken; the core of the personality is apt to be exposed. With a more acute course, primary physiogenic fear is likely to be present; the patient's need to explain his fear to himself may lead to delusions of persecution or to hypochondriacal beliefs. In other cases delusions may be primary and fear secondary. In later stages of progressive organic states psychological processes disintegrate and dementia results. The author states that schizophrenia is generally viewed as an organic disorder, but does not explain its pathology. He expresses the belief that the direct psychological consequences of schizophrenia have to do with alterations of the core of personality, namely attitudes towards the self, others, and the community. Some other pathological conditions are discussed by the author, but the discussion is too condensed for inclusion in this review.

Merlin's paper (84) is based partly on clinical and partly on experimental work with people who had severe traumatic experiences, e.g., loss of eyesight in war, or adverse vocational reclassification. Experiments on the disturbing effects of criticism of normal subjects by the experimenter are also reported. The experimental disorders thus produced were very similar in kind to the effects of the genuine traumatic experiences, but, unlike the latter, were very transitory.

The author views profound protective inhibition as the core of the traumatization syndrome. Only the stronger conditional reflexes are preserved. Automatized actions continue to function; remote goals cease to be effective. Misfortune blinds people. Patients tend not to believe reassurance. Therapeutic work making use of remote goals is apt to be ineffective. The author recommends a therapeutic program in which initially the work process itself is emphasized. Work provides interoceptive and proprioceptive stimulation which can be expected to improve the tonus of the cortex and gradually remove the protective inhibition. Work success is likely to begin to be appreciated during the restoration process; the patient becomes sensitive to his evaluation by the collective. The level of aspiration and of performance tends to rise. Late in the recovery process, intellectual functioning tends to be restored, remote effects of one's actions are understood, and remote goals become increasingly a factor in behavior.

A paper by Yarmolenko (85) discusses education of deaf-blind children in permanent collectives. It is stated that such collectives enable them to grow up as essentially normal socialized human beings, rather than as social isolates. Artistic and scientific activity of contemporary Russian deaf-blind is cited, particularly that of O. Skorokhodova, a deaf-blind poetess and author of a book on how she perceives the world.

The group of papers which have been just reviewed show that there is clinical work in the USSR, and that there are Soviet workers in this field who are aware of the role of the family situation and early childhood experience in adjustment. Merlin's utilization of the Pavlovian concept of protective inhibition illustrates the fact that this concept can be applied in many of the situations to which the concept of repression may also be applied.

Papers on motivation and personality.—There are additional papers which deal with motivational problems, mostly in the educational context; some deal with motivation in industrial situations. Kovalev (86) characterizes the development of character as consisting of three stages proceeding from situational determination of behavior to the establishment of firm convictions. Character development is also viewed as affected by the (Pavlovian) type of nervous functioning. The differences between the developmental stages are discussed as far as character reeducation is concerned.

According to Sobieva (87) self-descriptions by 11, 12, and 13 year olds indicate that children tend initially to accept other people's views of themselves and then progress to their own views, which then tend to become both more detailed and more generalized.

Chamata (88) discusses in general terms the large number of situations (games, shop work, literary readings, etc.) which contribute to the developing self image of the child. Perov (89) reports on the development of vocational interests in children; the results are the expected ones. Interests are likely to increase in permanence with age and to be more influenced by the personal and social meaning of the activity than by the example of particular persons.

Podberezin (90) presents theoretical considerations about interests, including a distinction between interest as amusement and goal-derived interest; he also summarizes the results of an experiment on conditional responses to interesting and indifferent conditional stimuli. Dobrynin (91) discusses the social consequences of one's actions as contributing to motivation. Prosetsky (92) views imitation as an important factor in personality development. He accepts Pavlov's view of imitation as an "unconditional reflex" and states that it has been shown that the development of imitation in children proceeds from unconscious to conscious, from imitation as an end to imitation as means to an end, from playful to practical, and from imitation of proximal models to that of distant models.

Varnakova (93) summarizes the results of a large-scale study on the effects of the process of group decision on the personality of workers. The study was conducted in a number of factories over a period of years. According to the author, most practical decisions are made in factories by workers' collectives. The effects of the process of group discussion and decision are described as favorable: group problem-solving is effective, group allegiance of workers is improved, their self-confidence is increased, etc. Menshchikova (94) discusses the development of initiative in industrial workers. She states that most workers tend to show initiative by introducing innovations into the work process; most of those who do not can be trained to do so if given tasks of the correct degree of difficulty.

Aptitudes.—Several papers deal with aptitudes, two of them with artistic aptitudes. Kireenko (95) presents a theory of aptitude in graphic arts, also a test. The theory emphasizes the artist's ability to perceive a visual situation as a whole. The relationship between the theory and the test is not made clear in this paper, but additional information on this issue is presented in a later publication (96). In the test, subjects were asked to arrange a set of 3-dimensional geometric figures so as to reproduce a previously-exposed model. The test was given to two groups of 7- to 15-year-old subjects, one group with artistic inclinations and the other constituting a control group. The measures of error in reproducing the set of geometric figures were on the average very much smaller in the artistically inclined group.

Drankov (97) suggests that creativity depends on the joint effects of a number of abilities and emotional traits. Great artists are apt to exhibit more than one aptitude. The great Russian poets Pushkin and Lermontov showed talent in graphic arts and evidence is presented indicating that both of them utilized their drawing or painting in the process of literary creation.

Platonov (98) discusses aptitudes of fliers. It is suggested that aptitudes are not fixed,¹¹ but nevertheless are sufficiently stable for practical purposes. The paper summarizes conclusions based on varied experimental and observational procedures. The author lists a number of traits which are desira-

¹¹ Russian psychologists are apt to attribute to foreign psychologists the point of view that aptitudes are innate and fixed.

ble in fliers. Some of these are: a strong, balanced, mobile type of nervous system; persistence; decisiveness; initiative; interest in flying; ability to distribute attention; ease of shift of attention; stability of attention; emotional stability; good coordination; etc. However, no two fliers are alike; they do not have to have all the desirable traits; desirable traits function in patterns, and the absence of some may be compensated by others. An attempt was made to predict the performance of students in a flying course. A number of experimental and observational procedures were used, but are not described in the paper. For each subject, the pattern of results was evaluated "clinically." A table is presented indicating the relation between the predictions and a four-category classification of the performance in flying instruction apparently as graded by the instructional staff. The author states that the table indicates that the findings offer some promise, although the prediction of flying performance was by no means perfect. Actually the table suggests a correlation of about 0.75,¹² which is higher than most validity coefficients in aptitude testing. However, the number of cases is not given, and it is not mentioned whether or not the graders of the flying students were ignorant of the predictions. The apparently high correlation may indicate a promising method of aptitude determination, or it may be a result of errors in methodology. The work of the author suggests that aptitude testing¹³ may see a revival in the U.S.S.R., with more emphasis on what Viteles (99) called analytical tests presupposing a theoretical understanding of the performance to be investigated, than tends to be the case in this country.

The total number of papers included in the published proceedings of the Conference on Individuality was thirty-seven.

PAPERS IN *Questions of Psychology*

The largest category of papers in the Russian psychological periodical, *Voprosy Psikhologii* (*Questions of Psychology*), which began to appear in 1955, deals with educational topics. Not all of the papers represent new material. Some of the papers appear to present in greater detail much the same material which was already presented in summary form at the 1953 or the 1956 conference. Other papers extend similar principles to other topics. Still other papers deal with new material.

Education.—A paper by Slavina (100) reports an attempt to create habits of punctual, neat, and accurate homework in children in the first grade of school (7-year olds). The procedure involved class meetings at which the children were told by the teacher that at school they had to learn to do all their work well, and that therefore everybody whose work was not perfect had to do it over after school hours. According to the author, the children

¹² No exact correlation can be computed, because the results are presented in percentages by rows instead of by frequencies.

¹³ The author would undoubtedly deny that he made use of tests, in view of the distinction made in the U.S.S.R. between study of persons by means of tests and by psychological experiments (10).

tended to accept the explanation and to view the work after school hours as an opportunity to learn to do better work; but the procedure works only if no exceptions are allowed and if the work after school is not presented by the teacher as a punishment. In a class where the latter happened no good work habits developed. In two classes in which the teacher proceeded in accordance with the instructions, inadequate work practically disappeared within a few weeks and the children developed excellent work habits which persisted for years. A paper by Bozhovich (101) presents some of the same and additional similar material.

A number of papers appear to deal with a somewhat interrelated group of problems. Menchinskaya (102) presents material indicating that school children who know certain rules, e.g., of geometry, are apt not to think of these rules when presented with a concrete problem. She suggests that this is the case because the rules were learned out of contact with real objects. Yakobson (103) describes a rather similar inability to connect knowledge of rules with the ability to deal with concrete objects in the case of teen-age boys dealing with technical problems. Petrova (104) made similar observations on spelling and the spelling rules. Galperin & Talyzina (105) report how children who had been viewed by their teachers as hopeless in geometry were taught the meaning of geometric concepts and principles by being given practice in deciding in terms of prepared criteria whether concepts like "straight line," "perpendicular," etc. applied to particular concrete objects. This work represents an application of the same idea to geometry which had been presented by Galperin and by Slavina at the 1953 conference in relation to arithmetic. The facts pertaining to the learning of arithmetic are presented in some detail by Davydov (106) and by Nepomniashchaya (107); the latter investigator appears to have taught some understanding of simple numerical operations to the feeble-minded, including imbeciles. The common feature of this group of studies is the idea that abstract knowledge is ineffective in itself and becomes effective only in the course of concrete actions.

There are additional educational papers. Ginzburg (108) investigated children who were viewed by their teachers as exhibiting different levels of scholastic abilities; their performance on examination questions was studied and the investigator discussed the course content with them. The results indicated that the children who were thought to be deficient in ability lacked the proper habits of analysis and synthesis. An attempt was made to train them in such habits by giving them assignments and by subsequent discussion. Favorable results are reported. Lublinskaya (109) showed that children's descriptions of pictures (e.g., of a lion or tiger) are markedly affected by verbal information about the objects depicted; thus the children are much more likely to notice a lion's sharp claws if they are told a story about the lion's hunting habits. Names functioning as labels may also aid the child in noticing and recalling information items. A study by Kabanova-Meller (110) presents facts on the effects of illustrative material in instruction which are similar to those presented by Menchinskaya at the 1953 conference. Studies

by Zhuikov (111) and Karpova (112) deal with various aspects of the development of the consciousness of words as units of speech. Sinitsa (113) studied the ways in which older children discover the meaning of unfamiliar words within a context. Poliakova (114) studied the problem of effective study of different but related rules of grammar. She found that when the children learn rules separately they tend to remember them but do not tend to apply them correctly; when they are contrasted during the learning process, children tend to confuse them in recalling them; the best results are achieved when rules are learned separately and later contrasted with each other. Kadochkin (115) and Vishnepolskaya (116) studied the role of various aspects of rote memory in learning to spell. Orlova (117) showed the ability of 9-year-old children to distinguish between sentences which are semantically complete or incomplete. Bubnova (118) reported on individual differences in the degree of understanding of stories by children; the individual differences are discussed in Pavlovian terms. Elkonin (119) discusses problems related to phonic methods in teaching children to read. A paper by Kulikov (120) discusses the contribution of study of arithmetical operations to the development of the idea of reversibility of functional relations in mathematics.

Papers on topics related to education.—In two theoretical papers, in which educational research is used for illustrative purposes, Leontiev (121,¹⁴ 122) provides a theoretical rationale for Galperin's step-wise approach to teaching. Leontiev views psychological functioning as based on conditional reflexes or temporary bonds and their "locking together." He relates this view to his experiences in retraining the war injured. He views temporary bonds as functioning in complex systems and as hierarchically arranged; if an essential lower step is missing, the higher steps cannot be acquired. Intellectual skills are such higher steps, based upon lower steps which involve overt action. After the step-wise acquisition has taken place, the overt actions may drop out as a result of extinction; action becomes an intellectual performance, which is essentially a set of conditional reflexes with inhibited motor components. A paper by Bogoyavlensky (122a) which summarizes the results of a number of educational studies mainly in terms of the concepts of analysis and synthesis is also relevant. The viewpoint expressed in Leontiev's paper has important points of contact with a paper by Luria (123) about the development of the ability to follow verbal directions. Zaporozhets (124) reports on experiments similar to those reported by others at the 1953 conference on habit formation as related to verbal instruction and the opportunity to explore the situation. More or less related research dealing with interaction between speech, exploratory activity, and other tasks and showing that children initially cannot follow complex directions, that conditional responses are affected by the speech system and that exploratory activity is apt to help

¹⁴ The paper has also been presented at the International Congress of Psychology in Montreal in 1954.

in the learning of motor tasks may be also found in papers by Paramonova (125), Reinwald (126), Zinchenko (127), and Kisliuk (128).

Learning and conditioning.—There are a number of additional papers related to the theory of learning and conditioning. Asratian (129)¹⁵ presents the results of research by his associates on "switching" of conditioned reflexes. An animal, in the presence of one experimenter, can learn to respond to stimulus A with salivation and to stimulus B with withdrawal, while it can learn to switch around the conditional responses to the two stimuli in the presence of another experimenter. The experimental room or the time of the day may also function as the switching signal. The author states that the phenomenon of switching offers an explanation for the flexibility of adaptive behavior.

A paper by the physiologist Anokhin (130) presents his theory of the role of afferent impulses in conditioning. The theory is an elaboration of the Pavlovian views, that the cerebral cortex is mainly afferent rather than efferent in its function, and that the process of conditioning involves a "locking of a temporary bond" between two sensory centers, rather than the formation of a new stimulus-response connection. Anokhin suggests that afferent impulses informing the organism about the reaction that has taken place (reverse afferentation¹⁶) are involved in the sensory bond. He views the process of reinforcement as largely mediated by reverse afferentation; he characterizes chain reflexes as guided by it and believes that without this guidance chain reflexes would be rigid and nonadaptive. He points out that without reverse afferentation there can be no compensation for defects in the nervous system. He suggests that reverse afferentation plays a role in what he calls the "acceptor" function of the cortex. The organism tends to behave as if in initiating action it expects a certain outcome, as shown by changes in behavior if the expected outcome does not take place. Anokhin suggests that when action is initiated there develops a "preparatory complex of excitations" to which the reverse afferent impulses must correspond if the outcome is to be accepted for the organism. The author reports on the results of his experiments involving substitution of one food for another as unconditional stimuli in conditioning experiments (meat for biscuits). The dogs reacted with investigatory behavior, and indicated in other ways that they were responding to the lack of correspondence between the preliminary adjustment and the sensory impulses.

Meshcheriakov (131) and Merlin (132) used "motor conditional reflexes with verbal reinforcement" (a signal regularly followed by pressing a button in response to the command "press"). Meshcheriakov used chains of stimuli; he investigated the role of inner speech in the conditioning process. He found

¹⁵ This paper had previously been presented at the 1954 International Congress of Psychology in Montreal.

¹⁶ Anokhin points out that his theory of reverse afferentation was formulated long before the related notion about feedback.

that when allowed to talk, subjects often ask questions, make comments, and report inner speech. The author suggests that the speech system is not eliminated when the subject is not allowed to talk (as is customary), and that conditioning with verbal reinforcement is not simple formation of new bonds but involves complex interaction between habit systems one of which is that of speech. Merlin used a light of a certain color as conditioned stimulus; the name of the color was also presented, but not reinforced. In other experiments a color name was the conditioned stimulus while the light was not reinforced. In both experiments there were additional unreinforced colored lights and color names. The author tried to determine whether there would be a transfer of conditioning effects between colored lights and the corresponding color names. He found such transfer effects in all his subjects. But the results were complicated ones; in some instances, conditioning first appeared in the unreinforced system; in other instances the conditional responses appeared in one system as they disappeared in the other. The author suggests that Ivanov-Smolensky's theory of the conditioning process first taking place in the case of the reinforced system and then being "projected" to the other system is not verified by his results.

Vatsuro & Nikolsky (133) conducted delayed reaction experiments with normal children, morons, and imbeciles. Four cups were used; a piece of candy was hidden under one of them in sight of the subject, after which a screen was placed between the cups and the subject. Then the screen was removed and the subject was told that he could get the candy. An additional condition, namely a contrary habit, was used in a second series of experiments. A cup of a different color was introduced into the experiment; in a number of preliminary trials this cup was always the "positive" one; after this, in the delayed reaction trials the candy was hidden under one of the previously "negative" ones. Imbeciles were found to be capable of only short delays (from a few seconds to about 5 minutes). Morons were capable of much longer delays; no accurate measurements were made, but five-minute delays seldom caused difficulties, and some succeeded with delays of a week; but the performance of the morons tended to be seriously affected by the contrary habits. Normal children (7 to 11 years) were capable of very long delays (too long for convenient measurement), and appeared to be only slightly disturbed by contrary habits.

Krasilshchikova (134, 135) conducted two studies on the learning of verbal material. One of them deals with the well-known fact that items in the middle of a series tend to be learned later than items at the beginning or end of the series. The author interprets this as caused by cortical inhibitory processes; she reports that the effect tends to disappear in delayed recall; it also tends to be diminished if an excitatory drug (caffeine) is administered to the subjects; it tends to be increased if an inhibitory drug (chloral hydrate) is used. The other study demonstrates the effects of primacy, that is the relative superiority of material learned first over material learned later, by some new techniques.

Elkin (136) investigated what amounts to semantic conditioning. He

produced conditional withdrawal responses to 2- to 3-word sentences. Then he tested the subject's responses to the separate words. He reports that the results varied markedly from sentence to sentence and from word to word. According to the author, the result depends on whether the single words carry the "meaning load" of the sentence. In another study (137), the author reports a curious interference effect in verbal learning which may be characterized as conditional nonremembering. Word lists containing one very difficult word among ordinary ones were exposed six times to the subjects with the instruction to try to remember the list. The very difficult word tended not to be recalled correctly. Then a test series consisting entirely of ordinary words was presented to the subjects, also six times. The subjects tended not to remember the word occupying the same position in the series as the difficult word in the first series.

A paper by Ladygina-Kohts (138) deals with problem solving in animals, particularly in apes. A considerable amount of research by various authors is reviewed. The relative rigidity of performance of the animals and the situation-bound character of their behavior are stressed.

Several more papers deal with learning phenomena but will not be included in this review. Studies by Koltsova (139) and Maraev (140) impressed this reviewer as too specialized in factual content. A lively controversy appears to be in progress about the concepts "*navyk*" (habit-skill or practiced skill) and "*umenie*" (know-how). During the period under consideration, Rykov (141), Boiko (142), Hojava (143), and Yananis (144) have participated in the controversy. Two of the relevant facts are: that sometimes mastery of the task (know-how) develops only on the basis of much practice; and that sometimes the ability to perform a task develops without practice after one listens to an explanation or watches a demonstration. The controversy has to do with the exact meaning of the concepts "habit-skill" and "know-how" and the relations between them.

Pavlovian typology.—A number of papers deal with the Pavlovian types, mainly as modified by Teplov.¹⁷ Two papers by Teplov (58, 145) are largely restatements of his point of view which has been already reviewed. A similar point of view is also presented by Palei & Pshenichnov (146). A book by Teplov and his collaborators has been published and has been reviewed twice in the *Voprosy Psikhologii*. The research of Rozhdestvenskaya which dealt with the typological significance of the effects of extraneous stimuli on visual thresholds and which was presented by Teplov at the 1953 Conference has appeared as a paper (60).

Norakidze (147) suggests that the type of nervous functioning can be determined by means of experiments making use of the Uznadze size illusions; however, only a summary of his paper, originally presented at the 1955 Conference, is available in the *Voprosy Psikhologii*, and the nature of the technique for distinguishing between the types is not clearly presented.

Kossov (148) used his previously reported test of preponderance of the

¹⁷ Teplov has recently received an award for distinguished service in psychology.

first or second signal system in an investigation of the performance of school children in algebra. His results indicate a rather striking correspondence between the "type" as determined experimentally and the errors made by the children in algebra examinations.

Yakusheva (149) and Merlin (150) present case studies of children. Yakusheva presents nonexperimental case material indicating to her that temperament may change as a result of changed life circumstances; an editorial footnote expresses doubt about whether the author really deals with temperament in her paper. Merlin discusses the different reactions of two boys of contrasting type (ascertained experimentally) to a deterioration in their school grades. It is suggested that different temperaments require different educational methods.

Oriental reflexes.—Several papers deal with work related to that presented by Sokolov on "orientational reflexes" at the 1953 Conference. The material for the earlier paper is presented in more detail by Sokolov & Vinogradova (151). A paper by Sokolov (152) extends his research to other reactions, e.g., the EEG, the GSR, and visual thresholds.¹⁸ Related research has also been reported by Milerian (153) and by Maruszewski (154).

Sets and Uznadze size illusions.—A considerable number of papers deal with the concept of set as emphasized by the late Professor Uznadze, and with his size illusions. Most of these papers are theoretical ones and deal with various aspects of the question whether a psychology emphasizing the concept of set is compatible with Marxism; they will not be included in this review. But three papers which present psychological facts will be mentioned here.

Hojava (155) presents a new technique for the demonstration of sets. The technique makes use of the fact that many of the letters of the Latin alphabet also occur in the Russian alphabet and sometimes have the same meaning and sometimes different meanings. This affords the possibility of devising sets of ambiguous stimuli, words and nonsense words, which are read one way if the letters are perceived as Russian, another way if perceived as Latin. The author found it possible to establish Russian letter sets or Latin letter sets by appropriate instruction and practice with the nonsense words. Familiar words presented when the wrong set was in operation were apt to be read in accordance with the set and not recognized.

Bzhalava (156) summarized findings on sets in schizophrenics and epileptics. Apparently, Hojava's technique just referred to and other techniques were used. Characteristically different findings were obtained in the two abnormal groups, but only a summary of the results which have been presented at the 1955 Conference is available.

Wenger (157) investigated size illusions of the type studied by Uznadze and his school (e.g., Bzhalava) in an effort to determine whether their ex-

¹⁸ This paper has also been presented at the International Congress of Psychology in 1954 at Montreal.

planation in terms of sets is correct. He concluded that it is not and that, instead, the illusions are effects of changes in the perceived sizes of the training objects which occur as a result of their repeated presentation. He found that when two slightly different training objects are repeatedly presented the ratio between their estimated sizes tends to be larger than in reality, and this too-large ratio is usually accompanied by "assimilative" illusions, in which equal control objects are perceived as unequal in the same direction as the training objects.

When the training objects are sufficiently different from each other the ratio between their estimated sizes tends to be too small, and this finding is statistically associated with contrast illusions in the case of the control objects. The author presents additional findings which tend to show that the perceptual aftereffects of the repeated presentation of pairs of different objects can become conditioned to various features of the situation, and that these conditioned aftereffects explain certain results of the illusion experiments.

Reaction time.—Several papers deal with reaction times. A paper by Gurevich & Rozanova (158) deals with the relations between the intensity of the stimuli and reaction times. The reaction time to increasingly intense stimuli tends to be increasingly faster. In a discrimination reaction time experiment, an intense stimulus to which the subject is instructed not to respond tends to slow down the next reaction time.

Ushakova (159) investigated the reaction time to visual stimuli presented at different distances from the center of the visual field. The reaction time increased towards the periphery. She also determined the absolute thresholds and found a linear relationship between the logarithm of the threshold and the reaction time.

Chuprikova (160) reports results of experiments on reaction times which are much like those reported by Boiko at the 1953 Conference. The Bulgarian investigator Oshanin (161) reports on variability of reaction times. He reports that there are marked individual differences in variability, that the subjects with highly variable reaction times tend to have occasional unusually fast reaction times, and that these very fast reaction times tend to be immediately followed by very slow ones. The author interprets these findings in terms of induced inhibitory processes which follow intense excitation. He suggests that such processes should be characteristic of the nervous functioning of hysterics, states that the expectation was confirmed by experiments with several patients, and presents one example. He draws a similar inference about the effects of electroshock treatments in schizophrenics and presents an example confirming the inference.

The senses and sensory control of behavior.—Several papers deal with the senses, or with sensory control of activity. Beritov's (Beritashvili's) (162) experiments were conducted with dogs and cats in a large experimental room in which the animal was in a cage (or on a mat) and in which there was a food box. At a given sound signal the animal was taken out of the cage to the

food box, allowed to eat a little, and returned to the cage. After a few such trials the animals learned to go to the food box to the signal. When blindfolded, they initially tended to go to the sound signal, but learned to go in the correct direction of the food box after being taken there once or twice by the experimenter. If the location of the sound signal was then altered, the animals were initially disturbed, but could readily learn to ignore the location of the sound. Additional experiments and observations are cited which tend to show that the blindfolded animals did not (or did not have to) find their way to the food box by means of olfactory, tactile, or kinesthetic cues. The question of how the blindfolded animals did find their way to the food box arises. The author suggests that labyrinthine stimulation provides the answer. The author reports that experimental removal of nonauditory labyrinths abolished the animal's ability to find the food box blindfolded, and that they could not relearn it.

Bogush (163) briefly reports the results of a considerable number of different experiments, mostly on the reproduction of movements performed with and without visual control and under conditions of mirror vision. The main findings were to the effect that when a movement is perceived both by vision and kinesthesia the visual impulses play the dominant role. Movements originally produced under visual control can be reproduced more accurately under visual control than when the subject is blindfolded. Apparently, visual control interferes with utilization of kinesthetic information: movements originally performed blindfolded can be reproduced blindfolded more accurately than movements originally performed with vision and reproduced blindfolded. Vision also interfered with the reproduction of movements which were originally performed blindfolded. When in the original performance the visual field is reversed by the use of a mirror, the subject tends to reproduce what he saw rather than his movements.

A paper by Schwartz (164) presents a continuation of the research presented by her at the 1953 Conference.

Sokolov (165) investigated binocular interaction in dark adaptation. The visual thresholds were measured in peripheral vision. The author found that the dark adaptation of one eye results in partial dark adaptation of the other, and that this effect of the dark-adapted eye on the other one is considerably greater if the eye under the light-proof cup is kept open than when it is closed. The results are interpreted as involving reflex interaction involving eyelid muscles, the convergence mechanism, and the mechanism producing visual purple.

Teplov & Borisova (166) compared the "100 per cent thresholds" of pitch discrimination, obtained by use of the method of constant stimuli, with the size of intervals which enabled the subjects to use different judgment categories consistently when giving absolute judgments. Unlike some American investigators, the authors found relative judgment to be much finer than absolute judgment. The authors point out that the identification between sensory discrimination and Pavlovian differentiation of conditioned

reflexes, which is commonly made in Russian psychological writings, is shown to be erroneous by their results.

A paper by Kulagin (167) presents material on the influence of visual stimuli on sound localization. Sounds produced by invisible sources tend to be perceived as displaced in the direction of a dot of light projected on the screen. The author explains the result in terms of conditioning; temporary bonds are formed naturally between the visual images of sources of sounds and the sounds. The results with the dot of light are interpreted as involving generalization of the conditional response. The effect can be much enhanced by the use of the conditional response technique, with training trials in which the invisible source of the sound and the dot of light coincide.¹⁹ The author also presents an experiment in which a "soundless" earphone was used instead of the dot of light and the subjects were radio operators who are accustomed to hear sounds from earphones. The sound displacement effect was particularly great in this experiment.

Asafov & Smirnov (168) presented letters of the alphabet repeatedly to their subjects by nonvisual means. The letters could be written on the backs of their hands with a soft brush, or the hand of the subject was made to write the letter in the air, or the letter was "written" in the air by means of a source of sound. The subject was told after each presentation "say and write," and the technique is characterized by the authors as involving motor conditioning with verbal reinforcement. There were three groups of subjects. Five had normal vision (they were blindfolded in the experiment), five were blind and had lost their vision before they learned to read and write (they learned writing later), and five lost their vision after they learned to read and write. The results indicated that the blind since an early age had considerably more difficulties in recognizing the letters than the two other groups; in their case graphic reproduction of the letter tended to be ahead of the verbal naming. The seeing subjects and those who became blind late recognized the letters much more readily. In the late blind, graphic reproduction still tends to precede reading. In the normal group, reading tended to come first.

Applied psychology.—There are a number of papers dealing essentially with problems of applied psychology. Chebysheva (169) discusses the existence of consistent differences between industrial workers in work efficiency, seemingly with surprise. Differences between the efficient and inefficient workers in work methods, numbers of rest pauses, etc., are explained. Observations and laboratory experiments are described suggesting that a higher level of aspiration tends to result in greater interest in work and more efficient work methods; after practice for speed, the pace viewed as comfortable tends to become considerably faster than before such practice. A similar conclusion is reached by Karandeeva (170) on the basis of work with school children.

Dymersky (171) conducted an investigation on the effects of practicing

¹⁹ This experiment is also summarized by Leontiev (121).

flying in imagination on flying performance. He used himself as subject. He had taken a flying course and then not flown for four years. When he tried flying again with an instructor, his flying had markedly deteriorated. He took a number of lessons and then again stopped flying for a year. During this time he studied a manual for a different make of plane (the Yak 18) and regularly practiced flying it in imagination. Then he returned to flying instruction, flew a Yak 18 for the first time and, according to his flying instructor, did unusually well considering his limited total experience and long pause. The possibility of delayed practice effects is not discussed.

A study by Rubinstein (172) deals with workers making perforations in punch cards for use in business machines. The errors are examined and it is pointed out that most of them appear to be sensory rather than motor errors. They involve mostly numbers which are similar in shape rather than numbers with keys near each other on the machine. The author describes a new method of training the workers which appears to have been tried out with success.

Kireenko (96) presents the details of his research on art aptitude reported at the 1956 Conference.

Sokoliansky & Kulagin (173) describe in detail an electrical device for enabling the blind to read books in ordinary print. The apparatus translates letters into patterns (partly temporal ones) of raised dots. The authors state that the blind can readily learn to read print with the apparatus, which has been in use for over 20 years. A paper by Altschuler & Shapiro (174) deals with technical invention, but seems to this reviewer to be mainly concerned with invention from the engineering standpoint rather than with the psychology of invention.

There are a number of historical and theoretical papers which will not be reviewed here, e.g., a paper on the nature of the will (175), papers on the effect of the collective on the individual (175, 176), on attention (177), on the association concept (178). This reviewer found these papers to be too remote from the factual level, too abstract and programmatic to be of interest. There are also papers on psychology and Marxism.

Brain damage and behavior.—Two papers are neurological ones written by psychologists. A paper by Luria (179) discusses the great complexity of the afferent apparatus in the cerebral cortex and its role in motor control. Agreement with Pavlov's views on the presence of cells which are essentially afferent in function in the so-called motor area of the cortex is stressed. Some clinical findings are summarized.

Zeigarnik (180) presents two case histories of brain-damaged patients with a peculiar thinking-disorder. She interprets the thinking-disorder as inability to maintain goal-directedness in thinking and behavior. The patients were able to answer questions relevantly and often made a normal impression during conversations. But they showed remarkably little concern about their future or about their close relatives, forgot important tasks, and were not disturbed when their omissions or errors were pointed out to them.

When asked to state the meaning of proverbs they were apt to succeed; but when asked to pick out the right one in a multiple choice form they were apt to be side-tracked by superficial associations and to make foolish errors.

Other papers.—Several additional papers should be mentioned. Bozhovich's (181) paper deals with the developing self-image in teenagers. It overlaps in content with Sobieva's paper presented at the 1956 Conference, which has been already summarized. There is additional material from a study by Dragunova who asked her subjects to discuss certain literary characters. Bozhovich comments about the apparent absence of anxious brooding by Dragunova's subjects.

Rubtsova (182) also discusses evaluations of literary figures by her subjects. The emphasis in the paper is on the development of correct moral attitudes in school children and on the teacher's duty to promote them in instruction in literature.

Boiko's (183) paper deals with problem solving. His theory is similar to N. R. F. Maier's view of insight, but is formulated in terms of Pavlovian concepts. He suggests that a solution may involve a sudden interaction between conditional reflexes established earlier. He states that such a viewpoint has been already suggested by Ivanov-Smolensky's associates in 1934, but criticizes the older formulation for neglect of the speech system. Boiko describes an experiment he devised in order to illustrate the operation of the "interreflex" interaction. He made use of a panel with 16 lights, arranged in four rows and four columns and with 16 keys similarly arranged. The keys lit up the lights, but the spatial relationship between keys and lights was complicated. The subjects first were taught what groups of four keys had to be pressed to light up each of the four complete rows and the four columns. During the training period they always pressed the keys in groups of four. Then during the test trials the subjects had to light up single lights by pressing single keys; the problem could be solved in terms of the row and column at the intersection of which the lamp was located. Subjects were able to solve the problem, improved rapidly, and performed the tasks in terms of the interacting 4-lamp habits, rather than by memorizing single lamp-key combinations.

A paper by Zemtsova (184) discusses the utilization of their open eyes by blind persons formerly sighted. While sighted, they had acquired the habit of keeping their eyes open while performing many actions. These habits tend to be retained indefinitely after loss of vision, and the blind are likely to be disturbed in many actions if asked to close their eyes. Normal eye movements tended to occur while objects were being examined by touch.

Novikova (185) developed a technique for registering action currents from the tongue. She reports that they tend to be much increased during mental work. But this is not always the case, and tongue movements do not seem to be indispensable.

Lekhtman & Pechatnikov (186) conducted experiments in which subjects were required to solve arithmetical problems (or answer various ques-

tions) and simultaneously tap rhythmic patterns. In the case of the simplest rhythms there was apparently no interference between the two tasks. With the more complicated patterns there was. The authors explain their results in terms of Pavlovian concepts.

Vinogradova (187) worked on the plethysmographically recorded "orientational" vasoconstrictive reflex²⁰ in normal and feeble-minded children. She used words as stimuli and studied properties of the extinction of the reflex in the two groups. Striking differences between the groups are reported. In normal subjects, extinction of the reflex to a word tended to be gradual and new words continued to evoke the reflex. In the feeble-minded, extinction tended to occur much faster and more abruptly, and after a few new words it was likely to become so generalized that no words could produce it. After extinction, the reaction could be made to reappear if the subject was given the instruction to press a button in response to a particular word, e.g., cat. When other words were spoken, normal subjects did not make any overt errors, but tended to produce the orientational reflex in response to meaningfully related words, e.g., animal or kittens. The reflex did not occur in response to words with similar sound but unrelated in meaning. In the feeble-minded, words with similar sound do produce the reflex and the relation between meanings plays a lesser role than in the normal subjects. The author discusses differences between the higher- and lower-grade feeble-minded children as shown by this experiment.

A study by Kechkhuashvili (188) deals with visual imagery reported by listeners to music. There were differences in content, but there was a considerable amount of uniformity in the mood suggested by the imagery. Knowledge of the title of the piece (e.g., Schubert-Liszt's *Die Forelle*) tended to make the imagery more uniform and tended to increase the enjoyment of the listeners. Knowledge of a program of the piece (e.g., of the words of Schubert's song) tended to interfere with musical enjoyment.

PAPERS IN *Zhurnal Vysshei Nervnoi Deyatelnosti* (*Journal of Higher Nervous Activity*)

Study of "higher nervous activity" is the term which is used in the Soviet Union to designate research in the Pavlovian tradition. This tradition is separate from that of psychology. Pavlov viewed his work on conditional reflexes as research on brain physiology; conditional reflexes were considered by him as products of analytic and synthetic activity of the brain; studies of conditional reflexes, of the analytic and synthetic functioning of the senses, of speech, of the effects of brain injuries on behavior are studies of higher nervous activity. Pavlov has stated that higher nervous activity and behavior are the same. In the United States, much of the work on "higher nervous activity" is done by psychologists. In the U.S.S.R. it is largely done

²⁰ Compare the earlier account of Sokolov's paper.

by physiologists. Only part of this work can be included in this paper because of lack of available space and because of the very large amount of relevant material.²¹

A noteworthy feature of the *Zhurnal Vysshei Nervnoi Deyatelnosti* is the absence of irrelevant references to Marxism. The journal is clearly a scientific rather than a philosophic or a political one. When one of its editors (Asratian) wrote two papers on the philosophical (Marxist) implications of Pavlovian views of reflex function, the papers were published in the philosophical journal, *Voprosy Filosofii*. The biographical papers in the *Zhurnal Vysshei Nervnoi Deyatelnosti*, unlike most of those in the *Voprosy Psikhologii*, tend to deal with the scientific discoveries of the scientists, rather than with their political liberalism and philosophical materialism.

General papers on conditional reflexes.—A few papers in the *Zhurnal Vysshei Nervnoi Deyatelnosti* present more or less general characterizations of the nature of the process of reflex conditioning and its significance in explaining behavior. Kupalov (189), in a paper in which he criticizes Sherrington's semi-philosophical views on the nature of man, argues that the Pavlovian view of reflexes and conditional reflexes is very different from earlier views. Pavlov had developed methods for work on essentially intact organisms, rather than on decorticated ones; the latter work is legitimate for physiological analysis but not for study of normal unified activity of the organism.

The mechanism of unconditional reflexes does not function like a "Cartesian marionette"; it involves constant interaction with the environment, and adaptation to its changes; some of the reflex reactions appear to be as if the organism behaved in an anticipatory manner. Conditioned reflexes are apt to be anticipatory reactions; they explain foresight and adjustive character of behavior. In Orbeli's laboratory it has been shown that nest building is based on both unconditional and conditional reflexes.

Anokhin (190) emphasizes the need to understand the unified nature of organismic functioning in adaptive behavior. This functioning includes changes in vital functions, which are apt to anticipate the needs of the organism. The anticipatory changes of the vital functions are produced by complex mechanisms including the cerebral cortex and the reticular formation of the brain stem. They can become conditioned and form links in the systems of conditional reflexes of which behavior consists.

Papers on extinction, reinforcement, etc.—A number of papers deal with such topics as the probable origin of the phenomenon of extinction, the nature of reinforcement, the circumstances favorable to the formation of conditional reflexes, etc. Sokolov & Paramonova (191) worked with motor conditioning with verbal reinforcement. A sound was the conditional stimulus.

²¹ The preliminary notes of this reviewer included references to 106 relevant papers in the issues of *Zhurnal Vysshei Nervnoi Deyatelnosti* from 1955, No. 1, to 1957, No. 1.

Simultaneously, the authors recorded the galvanic skin reflex, which they view as an orientational reflex. The authors report that conditioning did not take place unless the orientational reflex was present.

Mokhova (192) investigated the EEG during the process of motor conditioning to a sound with verbal reinforcement. She reports the presence of slow oscillations with superimposed fast oscillations during the early period of conditioning. The effect is initially widespread in the cortex. As the conditional reflex becomes stable, these oscillations tend to become concentrated in the motor analyzer.

Skipin (193) attempted to deal with the question where the inhibitory process originates which results in extinction of a conditional reflex. Two opposed points of view had been previously expressed. According to one, inhibition which leads to extinction originates in the cortical representation of the conditioned stimulus; according to the other point of view, the inhibitory process originates in the central apparatus concerned with the reinforcement, e.g., the "food center." The author argues that if an animal has acquired a motor and a salivary response to the same stimulus and the inhibition originates in the center for the stimulus, the extinction of both responses should take place concurrently. His findings indicated that the extinction of the salivary response usually took place first, and the author concludes that the inhibitory process tends to originate somewhere between the cortical representation of the conditional stimulus and the food.

A different point of view on the origin of inhibition in extinction is presented in a theoretical paper by Kupalov (194).

Elshina & Zimkin & Moreva (195) conducted an experiment with mice in which an electric shock was begun and then a stimulus was given (half a second to a second later) which indicated that escape from the shock was possible; in some experiments this second stimulus was the lowering of an escape platform into the experimental box. The animals did learn to escape under these circumstances. The authors treat the shock as the unconditional stimulus, the second stimulus as the conditional one, and the escape of the animal as a conditional reflex. Since the unconditional stimulus came first in this experiment, the authors characterize the outcome as one involving a conditional reflex in which the conditional stimulus preceded the unconditional one. They discuss the question why this experiment succeeded where many other experiments failed. They explain their finding in terms of the fact that the conditional response resulted in a prompt cessation of the unconditional stimulus.

Complex stimuli.—A considerable number of papers deal with "complex stimuli" as conditional stimuli, or with related facts; the phenomena of "switching" of conditioned reflexes, and of relational learning represent such facts.

Chumak (196, 197) wrote two papers in which he presents some new experiments and theoretical considerations on relational learning. The author suggests that an animal learns to respond both to the absolute properties of

stimuli and to the relation between them when it is presented with a pair of different stimuli and the choice of only one of them is followed by a reward. The author criticizes Köhler for overlooking the role of absolute properties of the stimuli; their importance is shown by the fact that the positive and negative stimuli continue to be responded to in the same way when they are presented singly. The author also opposes Spence for not recognizing the reality of relational learning. He demonstrates it in an experiment in which the animals first learned to react positively and negatively to stimuli in pairs; in the second part the positive stimulus was presented alone and the reaction to it was extinguished; in the third part the two stimuli were presented together. The author reports that the animal then again responds positively to the stimulus, thus demonstrating relational learning. The author's experiments were conducted with rabbits, pigeons, and carp.

Several papers, mostly by Struchkov, deal with the switching of conditional reflexes. In one paper (198), this author reports a case of generalization of switching. Animals were trained by use of different reinforcements to switch their responses to two conditioned stimuli in a situation which differed from the original training situation. Responses to two other conditioned stimuli, which had remained stable in the original training situation, also switched spontaneously in the changed situation.

The expression "complex stimulus" refers to a number of stimuli presented simultaneously or in succession. A number of studies demonstrate the possibility of conditioned reflex differentiation between different orders of the same stimuli. Mering (199) reported experiments of this type on dogs with tone sequences which proved to be very difficult and resulted in experimental neurosis in some instances. Vediaev (200) reported success with some experiments on conditioning to complex stimuli with fish.

Kupalov (201) reviews several studies involving various kinds of interactions between the effects of conditional stimuli, unconditional stimuli, and additional features of the experimental situation. Two of the examples were: A dog had been trained to jump on a table and eat at the sound of a metronome. It was noticed that it occasionally shook itself while eating. Thereupon an attempt was made to reinforce the shaking by starting the metronome whenever the dog did it; the attempt succeeded. The author views the success of the experiment as an illustration of the unified character of the effects of the conditional and unconditional stimuli. Many American psychologists would have viewed the experiment as an illustration of secondary reinforcement.

In another experiment, dogs were trained to perform chains of successive acts so that the end of one act was followed by the signal to perform the next one (walking to the lamp; licking the lamp; experimenter turning on the lamp; dog walking to a rug; experimenter starting a metronome; dog jumping on a table; dog receives food). After the dogs had learned the chain of acts, a loud sound of a rattle was introduced. It is reported that if this sound begins near the end of the chain, while the dog waits for the metro-

nome, the dog tends to jump on the table at once. If the rattle sounds near the beginning of the experiment, the whole chain of acts is apt to be inhibited for several days.

Comparative study of conditioning.—Several studies appear to deal principally with the question whether certain conditional reflex experiments succeed with particular species of animals. Thus Bokov (202) attempted to obtain conditional withdrawal reflexes in pigeons; other investigators had failed to obtain them. He found that they were easily established if only the time interval between the stimuli was short enough. Popov (203) studied conditional withdrawal in horses and found results which were similar to those generally obtained with dogs. Some of the studies deal with ontogenetic development of animals. Possibly the most interesting one of these is by Moiseeva (204) who worked on puppies. An operation was performed on each puppy, in which a tube was inserted into its stomach so as to connect it to the surface of the body. Through this tube, a rubber balloon could be inserted and blown up rhythmically so as to provide a source of interoceptive stimulation. Conditional withdrawal reflexes were developed to rhythmic interoceptive stimulation and to the sound of a bell. It was found that in the younger puppies the conditional withdrawal to stomach stimulation developed very fast, faster than conditioning to the sound. In the older puppies interoceptive conditioning was considerably slower than in the younger puppies and slower than conditioning to the sound.

Studies of child behavior.—There are studies of child behavior. Orbeli's paper (205) is a review of the research done by his associates. One of the facts cited has to do with the conditioning of the plantar reflex in babies. As is generally known, the reflex initially occurs in the form of the Babinsky reflex and changes to its later form as the baby matures. Orbeli reports that the conditional reflex may be of this later kind at a time when the unconditional reflex still occurs in the form of the Babinsky reflex; and he also states that as the unconditional reflex loses the Babinsky form, the conditional reflex is likely to acquire it for a time. Other findings mentioned in the paper are very similar to those presented by Luria at the 1953 Conference about responses of young children to verbal directions. Still other findings have to do with the development of sucking in babies and with its inhibition by other stimuli which tend to arouse the orientational reflex.

A paper by Luria (206) summarizes a continuation of research presented by him at the 1953 Conference. The research appears to utilize some of Anokhin's ideas on reverse afferentation which have been already mentioned. The children were too immature to follow consistently the instruction "when the lamp lights up, press the button"; they could do it, however, if "sanctioning afferentation" was added by means of additional electrical connections and the children were told "when the lamp lights up, press the button and put it out," or "press the button and ring the bell." Instead of putting out the light or ringing the bell one could also ask the subject to use the self

command "must," or even to speak the nonsense word "too" (it is a nonsense word in Russian).

A paper by Krasnogorsky (207) summarizes the results of several kinds of research on the development of speech. The research included studies on the development of vocalization and vocabulary. Instances are presented of what would have been called semantic conditioning in the U.S.A. Material is presented on speech as related to the Pavlovian typology. A theory is mentioned in which stuttering is viewed as caused by interference of persistent foci of cortical excitation with speech, and there is reference to a method of therapy in which the subject is encouraged to speak whatever is on his tongue, so as to exhaust the foci of excitation.

Psychopathology.—A group of studies deals with psychopathology. Most of them report results of conditional reflex experiments with neurotics or psychotics or the feeble-minded. In some studies, association experiments are also used. Eyelid conditioning or motor conditioning with verbal reinforcement is the conditional reflex experiment which is usually used in these studies. Thus Usov (208) used eyewink conditioning and association experiments with middle-aged normal subjects, old people (67 to 96 years), and senile psychotics with delusions. Part of the time the two experiments were conducted together. The author reports that there was little interference between the two tasks in the middle-aged subjects, some interference in the old group, and much interference in the delusional group, particularly when words related to the delusions were used in the association experiment. The process of conditioning was unusually slow in the delusional group. The findings pertaining to the delusional group are interpreted in terms of an assumed persistent focus of cortical excitation which has an inhibitory effect on other cortical processes.

There are several more or less similar studies. One study (209) includes an account of successful conditioned desensitization to the stimuli which had functioned as triggers in cases of neurotic vomiting. Another study (210) reports an apparently novel utilization of hypnosis in three cases of hysteria. The symptoms are not described. The patients were given over periods of some months contrasting sets of hypnotic suggestions designed apparently to strengthen the functioning of each of the two signal systems. The hypnotized patients were told at times that they could feel and imagine but could not reason, and at times the opposite. The author states that all three patients recovered or improved.

In one paper (211) EEG changes during conditioning experiments while awake and while in deep hypnosis are compared and characteristic differences are reported. Also, hypnotized subjects were found to respond with similar EEG changes to the conditional stimulus itself, as for example a continuous bell, and to a verbal designation such as the spoken words "continuous bell"; indeed, the verbal designation tended to elicit even stronger responses. Several papers deal with the effects of hypnosis on conditional

reflexes and on verbal report; the findings are not particularly striking. A study by Hamburg (212) shows that suggestion may be a factor in drug effects on body functioning. Medical students were given caffeine and a barbiturate and were told that the caffeine was the barbiturate and vice versa; the effects of the two drugs on the blood pressure and heart beat corresponded to the names rather than the real drugs.

Senses.—Several other papers deal mainly with the senses. Gershuni (213) demonstrated conditioning to subliminal auditory stimuli. The threshold was determined by the method of limits, and the subliminal stimuli were 6 db below each subject's threshold. In another paper (214), the same author presents evidence that different methods of investigation (e.g., the method of verbal report, the conditioned reflex methods) give different thresholds. Roslavtsev (215) reported conditional changes in the size of the blind spot; colored light (red or green) was used as the unconditional stimulus. Vitenson (216) worked on afterimages as affected by sleep deprivation. Generally, sleep deprivation is said to result in a longer latent period and a shorter duration of the afterimage. But the results are complicated and are said to be affected by the type of nervous functioning. In view of the fact that investigations of the effects of fatigue by psychological techniques are usually negative, the author's findings are of interest.

Additional papers.—The above-mentioned categories of papers (each incompletely presented) represent a considerable sample of the material which concerns psychologists and which is to be found in the *Zhurnal Vysshei Nervnoi Deyatelnosti*. Three additional papers will be mentioned.

Dmitriev (217) presents facts which show that the technique of "motor conditioning with verbal reinforcement" is likely to lead to misleading results. The experiment usually succeeds with small normal children; but the percentage of failure of "conditioning" tends to increase with age in the normal population, and by the middle teens about half (or more) of the subjects give negative results; such negative results are in no way indicative of weakness or inertness of excitatory processes.

Maigorov & Firsov (218) report experiments in which chimpanzees had to learn to obtain incentives (candy or toys) out of an enclosure with an opening away from them by the use of a stick. The learning took place initially in a trial and error manner. Eventually the animals learned to perform the solutions in a stereotyped manner. These stereotyped solutions tended to show a considerable amount of resistance to change when the setup was altered so as to make a shorter path available. The authors express doubts about the genuineness of insight solutions and suggest that apparent insight should be explained as utilization of old learning in new situations.

Beritov & Roitbak (219) present material on electrical phenomena in the cerebral cortex, obtained in part by inserting fine electrodes varying in length into the brain surface. They report that in addition to the electric oscillatory phenomena there exist also slowly developing electric potentials; in different cortical layers, these slow potentials are apt to differ in sign. The

authors review a considerable number of facts and arrive at the conclusion that the slow potentials have an inhibitory function. They suggest that they originate in small appendages of cortical dendrites.

CONCLUDING REMARKS

A number of characteristic features of the treatment of the facts of human experience and behavior in Soviet psychology have been already mentioned in this review. These features include a generally favorable view of emotion and much emphasis on interrelations between psychological functions; on conscious control of action; on the important role of theoretical convictions in behavior, emotional life, etc. In educational matters, there is much stress on the desirability of understanding on the part of the students. The stress on consciousness and cognitive factors in behavior is not contradicted by the acceptance of the conditional reflex as a basis of adaptive behavior. The conditional reflex is apt to be viewed not as a blindly functioning habit based on stimulus-response connections but as a flexible form of adjustment which enables the organism to respond to signals. Many writers treat the conditioning process as based on the "locking together" of temporary bonds between afferent processes. It has been also pointed out that the impression of near unanimity made by the textbooks is not valid; there are disagreements and controversies in Soviet psychology. Some additional features of Soviet psychology may be noted: London has pointed out repeatedly that Soviet psychologists make little use of statistics, seldom using any measures beyond means and percentages. It should be noted, however, that this lack seems to result from an attitude toward research which has compensating features. The Russians do not accept statistical trends on faith as affording final answers to problems. They are apt to demand experimental findings which will be capable of explaining individual cases. The result is, on one hand, that typological studies based on two or three subjects are considered seriously and in some instances receive much attention; on the other hand, a diagnostic procedure giving an apparent validity coefficient of .75 is considered only as affording some promise. The statistical naïveté appears to be combined with a high aspiration level as far as precision in the study of individuals is concerned.

The apparent lack of faith in statistical findings also seems to be a contributory factor to much of the educational research of Russian psychologists. In the United States, one often finds statements in the literature to the effect that if a certain educational objective is typically not achieved in schools, it is not a suitable objective. In Russia, such a finding is likely to lead in some instances to confidently-offered recommendations for untested educational changes; but, in other instances, extensive and apparently valuable research on educational processes has been accomplished.

Certain issues not primarily related to the state of psychology in Russia are of interest because they appear to reflect aspects of contemporary Soviet culture which seem unexpected. One such aspect is the high regard with

which democratic political theorists of the 19th century are generally viewed in Russia. The Soviet Union is a one-party dictatorship. And yet, Russian psychological literature abounds in references, invariably favorable, to liberal writers of the 19th century. Razran has expressed the idea that Pavlovian theories may function as a Trojan horse introduced into Communist ideology. It seems to this reviewer that the writings of 19th century liberal Russian political theorists are much more appropriate Trojan horses than the nonpolitical theories of Pavlov.

There are curious examples of conservatism in Soviet culture. In one of the psychology textbooks, published in 1950, there are four photographs of actors illustrating emotional expressions; three of these actors were already famous when this reviewer was a boy in Moscow in the second decade of this century. At a meeting devoted to the memory of the 19th century physiologist Sechenov, attended by 300 people, all the papers were devoted to Sechenov's own work. In Sovietized East Germany, at the University of Leipzig, a committee for the study of the psychological heritage of Wilhelm Wundt was established (220).

There is a story in Yakobson's book on emotions which suggests an unexpectedly tender-hearted attitude on the part of Russian educational psychologists. Some students in a military school were caught cutting up a pillow and scattering feathers in a superior's quarters. The ring-leader was treated with what the author calls "cruel but strong means." The "cruel" punishment consisted of loss of military insignia for two weeks, of having to march alone behind and beside his unit instead of with it, and of an announcement of his misdeed to all the companies. A number of stories in Russian psychological literature suggest similar attitudes toward children on the part of psychologists.

ACKNOWLEDGMENT

This reviewer wishes to thank Dr. I. D. London for his courtesy in making available some of the material on which this review is based.

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²² An alternative transliteration, possibly phonetically preferable.

²³ Transliteration in accordance with the system used by the Library of Congress.

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